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Observations on the Effects produced by the Bile, in the Process of Digestion, in a Letter to the Editor. By B. C. BRODIE, Esq., F. R. S., Professor of Anatomy and Surgery to the Royal College of Surgeons, &c.

(From the Journal of Science, Literature and the Arts.)

DEAR SIR,—I send you a brief history of some experiments relating to the uses of the Bile, which I made a few years ago, and which I mentioned in my lectures delivered in the Theatre of the Royal College of Surgeons during the last spring. They form a part of a series of experiments relating to the function of digestion. I hope on some future occasion to be able to lay the rest of the investigation before the public, but it is not in a state of sufficient forwardness for me to venture to do so at present.

Various opinions have been entertained by physiologists respecting the office of the liver. Some have supposed that the secretion of bile is merely excrementitious; others that the bile is intended to stimulate the intestine, and to produce a ready evacuation of the fæces; and another opinion has been, that the bile is poured out into the duodenum, that it may be blended with the chime, and, by producing chemical changes in it, convert it into chyle. The situation of the liver, connected as it is in every instance with the upper part of the alimentary canal, is unfavourable to the first of these hypotheses; but the last is rendered very probable by the circumstance of chylification taking place just at the part where the bile flows into the bowel.

In order that I might arrive at some satisfactory conclusion on these points, I applied a ligature round the choledoch duct of an animal, so as completely to prevent the bile entering the intestine, and then noted the effects produced on the digestion of the food which the animal had swallowed, either immediately before or immediately after the operation. The experiment was repeated several times, and the results were uniform. Before I describe these results, it may be proper to make one further observation. The application of a ligature round the choledoch duct is easily accomplished, and with very little suffering to the animal; so that any derangement in the functions of the viscera, which follows, cannot reasonably be attributed to the mere operation. The division of the stomachic ropes, or terminations of the eighth pair of nerves on the cardia of the stomach, and the ligature of the whole extremity of the pancreas, are operations of much greater difficulty; yet it has been ascertained that neither of these at all interfere with the conversion of the food into chyme, or that of the chyme into chyle.

When an animal swallows solid food, the first change which it undergoes is that of solution in the stomach. In this state of solution it is denominated *chyme*. The appearance of the chyme varies according to the nature of the food. For example, in the stomach of a cat the lean or muscular part of animal food is converted into a brown fluid, of the consistence of thin cream; while milk is first separated into its two constituent parts of coagulum and whey, the former of which is afterwards redissolved, and the whole converted into a fluid substance, with very minute portions of coagulum floating in it. Under ordinary circumstances, the chyme, as soon as it has entered the duodenum, assumes the character of *chyle*. The latter is seen mixed with excrementitious matter in the intestine; and in its pure state ascending the lacteal vessels. Nothing like chyle is ever found in the stomach; and Dr. Prout, whose attention has been much directed to the chemical examination of these fluids, has ascertained that albumen, which is the principal component part of chyle, is never to be discovered higher than the pylorus. Now, in my experiments, which were made chiefly on young cats, where a ligature had been applied so as to obstruct the choledoch duct, the first of these processes, namely, the production of chyme in the stomach, took place as usual; but the second, namely, the conversion of the chyme into chyle, was invariably and completely interrupted. Not the smallest trace of chyle was perceptible either in the intestines or in the lacteals. The former contained a semi-fluid substance, resembling the chyme found in the stomach, with this difference, however, that it became of a thicker consistence in proportion as it was at a greater distance from the stomach:

and that, as it approached the termination of the ileum in the cæcum, the fluid part of it had altogether disappeared, and there remained only a solid substance, differing in appearance from ordinary fæces. The lacteals contained a transparent fluid, which I suppose to have consisted partly of lymph, partly of the more fluid part of the chyme, which had become absorbed.

I conceive that these experiments are sufficient to prove that the office of the bile is to change the nutritious part of the chyme into chyle, and to separate from it the excrementitious matter. An observation will here occur to the physiologist. If the bile be of so much importance in the animal economy, how is it that persons occasionally live for a considerable time, in whom the flow of bile into the duodenum is interrupted? On this point it may be remarked, 1st, That it seldom happens that the obstruction of the choledoch duct from disease is so complete as to prevent the passage of the bile altogether; and the circumstance of the evacuations being of a white colour, may prove the deficiency, but does not prove the total absence of bile. 2dly, That in a very few authenticated cases, which have occurred of total obliteration of the choledoch duct in the human subject, there has been, I believe, always extreme emaciation, showing that the function of nutrition was not properly performed. 3dly, That the fact of individuals having occasionally lived for a few weeks or months under these circumstances only proves that nutrition may take place to some extent without chyle being formed. In my experiments I found that the more fluid parts of the chyme had been absorbed, and probably this would have been sufficient to maintain life during a limited period of time.

In the prosecution of this inquiry, a circumstance occurred, which seems not unworthy of notice, although not immediately connected with the subject of digestion. The ligature applied round the choledoch duct was always a single silk thread, the ends of which were cut off close to the knot. If the animal was allowed to live, he became jaundiced. The *tunicæ conjunctivæ* of the eyes were tinged with bile, and bile was seen in the urine. But at the end of seven or eight days, I found, in several instances, that an effort was made by nature to repair the injury done by the operation, and to restore the passage of the bile into the intestine. In these instances, on destroying the animal at the end of the above-mentioned period, and exposing the cavity of the abdomen, and then making an opening into the duodenum, I ascertained that on compressing the gall bladder the bile flowed out of the orifice of the choledoch duct in a full stream, in spite of the ligature. On further dissection, I found that a mass of albumen, (coagulable lymph,) had been effused, adhering to the choledoch duct above and below the ligature, and to the neighbouring parts, and enclosing a cavity

in which the ligature was contained. The pressure of the latter had caused the duct to ulcerate, without adhesion having taken place of the surfaces which had been brought into contact; and the ligature, having been separated from it by ulceration, lay loose in the cavity formed by the albumen which had been effused around it. Into this cavity the bile might be made to flow from the upper orifice, and out of it into the lower orifice of the choledoch duct; and thus the continuity of the canal intended for the passage of the bile was restored. It is still more remarkable that the same thing happened even when two ligatures had been applied on the choledoch duct at some distance from each other.

The physiologist will not fail to observe the difference between the effects produced by a ligature applied to an excretory tube and a ligature applied to an artery or vein. A circumstance nearly corresponding to that which I have now mentioned, has been noticed by Mr. Travers, respecting the consequences which follow the application of a ligature round the intestine.*

I remain, dear Sir,
Yours, &c.,

December 9, 1822. B. C. Brodie.

Remarks on the Tonic Treatment of Phthisis, with cases of Dyspeptic Phthisis, by John Hume, M. D. *&c. &c. Read to the Clydesdale Medical Society, December* 6, 1822.

(From the Quarterly Journal of Foreign Medicine and Surgery.)

Although an infinity of essays have been written on Phthisis, some of them by men of distinguished talents; I beg leave to offer a few remarks on that subject, in which I have no intention to trouble you with a minute detail of cases or dissections, but merely to give the result of my experience in that disease for the last twenty years.

In the neighbourhood of Hamilton, which Dr. Cullen, with a pardonable partiality, used to call the Montpellier of Scotland, and whither he has been known to send his consumptive patients, the cases of Phthisis which occur are not numerous, and this is no doubt in a great measure owing to the comparative mildness of the climate. The mean temperature of the air is about 50° of Fahrenheit. There are no hills in the vicinity of the town, but the country rises gradually all round it for some miles, to the height of from four to eight hundred feet above the level of the

* See an Inquiry respecting the Process of Nature in repairing Injuries of the Intestines. By B. Travers. London, 1812.

sea; except in a north-west direction, where the Clyde proceeds towards the Atlantic. The prevailing wind, perhaps for nine months of the year, is from the south-west; and in the months of November, December, and January, it is sometimes very high, and attended with much rain. The frost seldom continues long, and occurs generally in December and January. The mean height of the barometer is 29½ inches.

The origin of Phthisis, for the most part, is attributed, in this neighbourhood, to cold; but it is not those persons, of either sex, who are most exposed to the vicissitudes of weather, that are most liable to its attacks. It is the female who is generally confined to the house by some sedentary employment, or the sedentary artisan, who most frequently become its victims; whilst those escape, as I have just said, who are much in the open air and in constant motion. Yet this fact, though forcibly pressed upon our observation, had, at one time, and still has, in many instances, little effect upon our practice. Instead of imitating nature, and inuring the patient, who is threatened with Phthisis, to exercise in the healthy breeze of the open day, we too often confine him to the house and consign him to inactivity, and a stagnant, and perhaps an unwholesome atmosphere, especially if he be of the lower order. And to make the disease more speedily fatal, we put the patient upon a spare diet, and farther destroy the tone of his stomach with deleterious drugs, particularly digitalis. In short, we believe the disease to be inflammatory, and not in a chronic but an active form; and even in its last stage, we but too often draw out against it the whole array of the antiphlogistic regimen.

Such were my views of Phthisis fifteen years ago, and I was so impressed with the fatal nature of hectic fever, that I was apt, at least mentally, to give up all hope of the patient who was subject to it. E. W. an unmarried lady, betwixt 30 and 40 years of age, had, during the winter of 1807 catarrhal symptoms, attended with slight hæmoptysis. In March of that year, she was visited by a late eminent Surgeon in Glasgow, to whom, a few days afterwards, I wrote concerning her, as follows.—"She is weaker than when you saw her, still loathing every kind of food, and taking little or nothing to support her but the wine. The only sleep she obtains is from the opiate. She is generally chilly through the day, and always in the night, till a sweat, breaking out towards morning, relieves her. She still complains of pain and tightness in her right side. The cough, however, is not severe, and at present she has no purulent expectoration; but the spitting of blood has returned. Her pulse is 120 and feeble. Her bowels are always bound, except when moved by the Pil. Aloes c. Myrrha. She has taken four grains of prepared steel every day since you saw her, and a teaspoon-

ful of Tincture of Columbo; upon the whole I see no reason to alter my opinion that she is consumptive, and that there is little hope of her recovery. I gave you my reasons for agreeing to the tonic plan of treatment, which I believe the doctrine of the schools has condemned in Phthisis." This lady continued for some weeks in a very precarious state. Her fever, in a short time, assumed a very distinct tertian type, returning every second day at the same hour, with a severe shivering, which lasted about fifteen minutes. Purulent expectorations, alternated with spitting of blood. Her morning perspirations were not lessened. She became extremely weak and emaciated; yet she could not be prevailed upon to keep her room, but always came down stairs to the parlour. The tonic plan of treatment was continued. As soon as the first daisy appeared on the ground, I walked out with her for a few minutes, and had her out every day, till her appetite began to improve, and she could extend her walk to some distance. Then I mounted her on horseback, and persevered through the summer with that exercise, although I saw her frequently spit out blood during the ride. Dr. Baillie having been consulted on her case, recommended the warm salt water bath and change of scene. This was complied with, and she gradually recovered health and strength; and the patient, whose life I once despaired of, is now a stout fat woman.

D. M. an officer of the Royals, a young man of a somewhat corpulent appearance, was seized with very profuse hœmoptysis, in the winter season of 1804. As his pulse was strong and quick, he was bled largely, and took the acidulated infusion of roses, with Saline laxatives. Under this treatment the hœmoptysis disappeared, but having in a short time returned, the same treatment was repeated, and with the same success. By this time the patient was much reduced. Hectic fever made its appearance, and a very profuse purulent expectoration. He became very deaf. At this time a Staff Surgeon, who had once been in the Royals, visited him, and proposed the employment of digitalis; but as I objected to that medicine, he prescribed some mild pectoral medicine. Merely as an experiment, I now put him on the use of calomel, combined with prepared chalk; of which he was to take a powder twice a day, containing a grain of calomel and two grains of chalk. Although constantly confined to bed, he had a keen appetite; and thinking the case desperate I allowed him to eat any thing he pleased; but advised him to live chiefly on milk. He complied with my advice so earnestly, that for some weeks, he took the almost incredible quantity of eight quarts of milk every day. He took three dozen of the above mentioned powders; but without having his mouth in the least affected by them. Occasional opiate and pectoral medicines were administered. As the spring ad-

vanced, his hectic symptoms abated, and in May I had the pleasure of mounting him on horseback, and, in a few months, he recovered nearly his usual health. He soon after sailed with his regiment to the West Indies, and in a few years I had the pleasure of seeing him again at Hamilton in perfect health.

L. A. a married lady, in her 65th year, had been for a long time in an infirm state of health, principally from chronic Rheumatism. She was seized with giddiness in March, 1816, which continued till the latter end of June, with little or no abatement, in spite of the various remedies employed. She was now seized with feverish symptoms, attended by cough, soreness of her mouth and throat, and difficulty of swallowing. Although her fever was violent, the emaciation and debility of her frame were so great, that I dared not use general blood-letting. However, I used the antiphlogistic regimen to a certain extent, applied leeches and blisters to her throat, and used gargles. The mouth, tongue, and fauces were now covered with a whitish slough, and swallowing became so painful and difficult that I was apprehensive of fatal consequences, particularly as the cough was still severe, and respiration by no means easy. The mind, however, was unaffected. In addition to the above symptoms, she complained of pain in her chest. I then determined to put my trust in blisters, and applied them repeatedly to various parts of the thorax, till the threatening symptoms were subdued, and the mouth and throat began to look clearer. Pectorals were administered, but not in great quantity, as the patient had a natural irritability of stomach which made her very readily loath such medicines.

By the 20th of July, the feverish symptoms were greatly lessened, but there was much cough, and an expectoration of purulent matter. She was also much troubled with flatulence, and there was an entire loss of appetite. At this time the rheumatic pains had left her. For her cough and spitting, and to procure sleep, I prescribed pills at bed-time, composed of opium, kino, carbonate of ammonia, and extract of chamomile; and to improve her stomach and general strength, I gave her bitters, prepared with gentian, colombo, aromatics and carbonate of potass. Her bowels were kept regular by gentle laxatives. To repress feverish irritation, and check the spitting of purulent matter, she was ordered pills, composed of kino and alum, but her stomach could not bear them. By the beginning of August she was gradually introduced to the open air, first in a carriage and afterwards on foot, and by the 20th of October, the expectoration was almost gone, though there was still a great deal of cough and debility. I now prescribed for her pills, composed of extract of gentian, carbonate of iron, and aromatics; and under their use she not only recovered her ap-

petite, but gained a much greater degree of strength than she had possessed for some years. They were continued till the latter end of November, when, unfortunately, the patient caught a fresh cold. During December, her cough was exceedingly severe, and came on in paroxysms, at uncertain periods, with purulent expectoration, at times streaked with blood. Her pulse varied from 68 to 90. There was a degree of chilliness occasionally, but no distinct hectic fever. A full inspiration produced a fit of coughing, but its sound, to my ear, did not indicate that the cells of the lungs were much stuffed; nor did any pain attend the inspiration. Her appetite was rather good, but her emaciation great. She slept well, but that she attributed to the pills of opium and kino, which she had never omitted since her summer attack. She had one or two blisters put to her chest, and she used gentian and colombo in infusion. Her bowels were kept open by the Pil. Rhei. Comp.

In January, 1817, I drew up a statement of her case for Dr. Gregory, who prescribed hemlock pills and decoction of bark. The pills did not agree with her; but she took some of the bark decoction, and as the spring advanced, she gradually recovered, under the use of steel and bitters. For her cough, which continued to tease her very much, I found nothing so beneficial as a mixture composed of Acetum Scillæ, Flores Benzoini, Nitrous Æther and Syrup. It acted upon her like a charm. During the summer, she took much exercise, and went two journeys to the distance of thirty miles. She continued the same plan winter and summer, and enjoyed, excepting a slight cold occasionally, good health for some years. Yet she never could lay aside the compound opium pills, though she never increased the dose. At length, in the summer of 1821, when thirty miles distant from Hamilton, she was seized with feverish symptoms, and with pain in her side, for which a respectable practitioner bled her from the arm. I was called to see her; at which time she was in a state of languor, approaching to stupor, which carried her off in a few days; her vital powers being quite exhausted.

E. L., an unmarried lady, in her twenty-eighth year, of a family in which there have been instances of phthisis, consulted me in January, 1822. Though not of a very robust constitution, she has been enabled, till of late, to take much exercise in the open air, and often on horseback. Then she used to sleep 10 hours; but now hardly five; and her want of sleep at present is occasioned by a kind of restlessness, difficult to describe, but which is not owing to any sort of alarm. The heat of her skin is natural in the middle of the day, and she has no sensible perspiration at that time; but about seven in the evening, there is a considerable degree of heat on her skin, alternating

occasionally with chill, of about ten minutes duration, and followed, at length, by sensible perspiration, which, however, abates before she goes to bed, betwixt ten and eleven. During the continuance of the heat she is very thirsty. She has pain in short twitches, and sometimes for a whole afternoon, in the left hypochondrium; but on certain days she does not feel it; yet on drawing a deep breath, she is always sensible of a slight pain. She complains also of pain behind the left scapula, and of great weakness in her back. She has often pain all over her brow; and, at such times, the brow is generally very red. She has a dry cough, which is hard at times. The pulse is 80, and feeble. Her tongue is clean. She has very little appetite to breakfast; she can eat a little at two in the afternoon; but betwixt five and six, she can take no dinner. Her diet is always very plain. She has no immediate sickness after eating; but, an hour after, she is troubled with heart-burn. When the weather is fine, she walks out, about an hour every day, without being much fatigued; but, after walking half a mile, she is rather short of breath, and inclines to perspire. She also, sometimes rides on horseback or in a carriage. Formerly she could read aloud for a long while, and with ease; but now she never attempts it. Her bowels are generally costive. The menses appear at the usual time, but in small quantity; and generally there is a very slight appearance of leucorrhœa.

This lady used the vinegar and water friction, as recommended by Dr. Stewart; took laxatives of various kinds, so as to regulate the action of her bowels; was put upon alternate courses of steel and bitters, and took a good deal of exercise in the open air. She also had repeated blisters applied to her side, and at times she used the tartar emetic ointment, so as to produce numerous crops of pustules. In the early part of summer, though her health was much improved, she was sent off to Lisbon; whence I had the pleasure of seeing her return, in the beginning of October, in perfect health.

I could give many more instances of recovery from purulent expectoration and hectic symptoms by the use of tonics and well-regulated exercise in the open air, were it at all necessary. Cases, however, have occurred to me, as they must have done to every man who has practised medicine for some time, which at first did not appear in the least unpromising, but terminated fatally in a very short time. Hence the young practitioner should use every precaution against this deceitful malady; for in no one is it more necessary, *principiis obstare*, than in this.

Thus phthisis, in its incipient state, will sometimes, for months, assume the appearance of a stomach affection, attended with debility, languor, headach, giddiness, flatulence, nausea, vomiting, and costiveness; without cough or a single hectic

symptom; and whilst the physician is directing all his attention to the gastric symptoms, the real disease is making a very gradual but sure inroad on the constitution. Such appearances, however, occur most frequently in the delicate female in the higher orders of society, who is fond of sedentary amusements, and cannot easily be prevailed upon to breathe the refreshing gales of the country, but who will cheerfully sit up half the night at a ball or a music party, dancing, as Dr. Gregory says, in his strong language, into her coffin. Many of these I have known fall sacrifices to the disease; whilst, in the country, I have known a young servant girl, with an asthmatic cough, and an occasional spitting of blood, live for years under her complaint, and, perhaps, at length recover, although exposed every day to the vicissitudes of the weather. I have often recovered young persons of the middle ranks, who had been necessarily much confined to the house, by sending them twenty or thirty miles from home, and enjoining them to take exercise in every possible shape in the open air, and to use, at the same time, some of the most approved tonic remedies. But I presume that such treatment is familiar to most medical men.

I may here mention that I have known several instances of early pregnancy in unmarried females assume, most accurately, the appearances of incipient phthisis. Yet in these cases I have observed a kind of anxiety, or secret fretfulness of mind, which is very rarely a concomitant of phthisis. But here it is the absence of the menstrual discharge that is most complained of, or it is that which we are most earnestly solicited to restore; and physicians, in similar circumstances, cannot be too much on their guard; for I have known a case of pregnancy mistaken for a combination of phthisis and dropsy by one of the present ornaments of medicine.—"Honestæ etiam matronæ hac de re interdum decipiantur, et medicos ideo in turpem et periculosum errorem ducunt: innuptis vero, ex plebe saltem, de mensium defectu conquerentibus, propter causas satis manifestas nunquam temere credendum est."

In the treatment of phthisis, I wish we could attend solely to the *juvantia et lædentia*, and try not to be influenced by hypothesis, or at least by every new one which is introduced. In my opinion, with all our boasted acquirements, we have made very little improvement on the theory of Galen. He thinks that the acrid humour of catarrh, gradually descending into the lungs, first corrodes, and then ulcerates them. The concomitant fever of this state is hectic, which is to be subdued by the internal and external use of refrigerants. Such is his hot consumption, where wine or stimulants are evidently improper. But in his tabes frigida, which he likens to the emaciation and debility of old age, he recommends the warm bath and cordials.

In short, it would seem that his hot consumption, with hectic fever, is our incipient phthisis, with strong inflammatory diathesis, and his *tabes frigida* is phthisis, accompanied by emaciation and debility. Hectic fever, he tells us, may accompany the disorder of any internal organ, and is more or less curable in proportion to the greater or less expenditure of innate moisture; being altogether incurable when that is nearly expended, or, in other words, when the patient is reduced to a state of extreme emaciation or debility. These opinions will be found in Galen's Treatises de sanitate tuendo, de differentiis febrium, et de Marasmo.

Were it likely to be of any use, I could give cases where I have used blood-letting, digitalis, astringents, particularly kino, alum and superacet. plumbi, vinegar, and other acids, expectorants, balsamics, and tonics. It will be sufficient, I imagine, to mention the result in general terms. Unless in the first attack of hœmoptysis, when every thing indicated strong inflammatory action, I have always found that general blood-letting hastened the fatal termination of the disorder; whereas, local blood letting is often serviceable, if previously the patient is not too much reduced. In one case, where hœmoptysis had returned with unusual violence, and when there was apparently strong arterial action, venesection was employed with immediate relief of the urgent symptoms, but in a day or two, purulent expectoration followed, with hectic fever, which continued till the patient's death. The same opinion may be given of digitalis. In inflammatory cases, and when the patient is not much debilitated, I have used it often with decided benefit; but when there is purulent expectoration and debility, it is worse than useless; and I fear that, in this country at least, he who expects from it the advantages experienced by Dr. Magennis will be disappointed, at least, I have been. In one case, when the pulse was 120, and feeble, and when there was much purulent expectoration, I gave it, by the advice of an eminent physician, from the 28th of March to the 11th of April, gradually increasing to 100 drops of the tincture in twenty-four hours. It was discontinued, on account of its causing pain in the stomach, want of appetite, and waking dreams; in fact, the patient, without knowing what it was, refused to take any more of it. It certainly lessened febrile action, and produced a great flow of urine; but in this, as well as every similar case I have used it in, there was great reason for believing that it hastened the patient's death. The trials made with the Prussic acid, in this neighbourhood, are not much in its favour.

Of astringents, I think that kino, when joined with opium, has often a very great effect in lessening expectoration and repressing febrile action; and in several cases, where I used su-

peracet. plumbi, in conjunction with opium, I was at first very sanguine in my expectations of a cure; but, in the end, it proved equally unavailing with other remedies. However, it seemed to produce no bad symptoms; and it evidently palliated the pectoral affection and its concomitant fever. The pills of Mons. Orban were also attended, for a short time, with the same good effect; however, they too are but palliatives. The same may be said of vinegar and other acids in various forms; of which, perhaps, the best is the infusion of red roses, acidulated with muriatic acid, and made palatable with sugar. I was led to the use of muriatic acid in phthisis, from having found that medicine eminently serviceable in scrofula, when taken for a long while in large quantities. An excellent pectoral mixture is made with treacle, honey, vinegar and laudanum. Syrups, prepared with balsams of Peru and Tolu, form a medicine agreeable to the patient; but they are merely palliatives; and the same may be said of all mucilaginous and oily mixtures. In some constitutions, the squill made into a balsam, with flowers of benzoin, nitrous æther, and syrup of sugar, seemed to have a wonderful effect in allaying, or even obviating, if taken in time, such coughs as harass the patient in severe paroxysms.

But of all the modes of treating phthisis, either incipient or confirmed, the tonic appears to me to be decidedly the best; and which, though it certainly cannot cure confirmed phthisis, prolongs the patient's life and renders him much less susceptible of those annoyances that unavoidably, for a long time, precede death. But, to be successful, this treatment must be commenced at a very early period. Then, provided there are no inflammatory symptoms, frictions, with cold vinegar and water twice a day, on the breast, or the whole body, till the skin is quite dry, exercise in the open air, or on horseback, in a carriage, on foot, or in a swing, according to circumstances; nourishing, but plain food, taken at such intervals as not to load the stomach, with the occasional use of steel and bitters, are the most proper remedies. Pain in any particular part may be removed by the application of leeches, aromatic embrocations, blisters, or the tartar emetic ointment. From the few trials which I have made with this remedy, I am inclined to have a very favourable opinion of it; for when pustules have been excited by it, they have never failed to give wonderful relief to the internal symptoms, although they themselves may have occasioned very considerable irritation. In one case, however, where even its frequent application failed to produce pustules, the patient complained of inexpressible anxiety and uneasiness in the epigastric region.

The greater part of this plan, I confess, is borrowed from Dr. Stewart; although, long before I knew how he treated phthisis,

I had appreciated the good effects of bracing remedies, and had employed many of them; and, in debility, or irregular action of the bowels, had often used, with the very best effect, a powder composed of crab's-eyes, rhubarb, nutmeg, ipecacuanha, and ginger, given so as to prove gently laxative.

Dr. Stewart was led to the peculiar practice which he has adopted in consumption, from reflecting attentively on some of Dr. Gregory's statements, when lecturing on that disease. In the opinion of that celebrated physician,—and indeed, of that of many others,—consumption, in the generality of cases, originates in a scrofulous constitution. What then is the cure, or the attempt at cure, in scrofula? Is it not to invigorate the constitution by a rich and generous diet, by exercise in a pure and open air, particularly near the sea-side, and by the cold bath? If such be the mode of cure directed for scrofulous ulcers on the surface of the body, is it not natural to believe that ulcers of the lungs, probably arising from a scrofulous cause, will be equally benefited by a similar treatment?

In cases where the skin glows with much heat, where the pulse is firm, strong, and quick, with severe topical pain, Dr. Stewart sees, at once, the propriety of using the antiphlogistic regimen; and in this he entirely agrees with the far greater number of physicians. But, would he do so when the patient is enfeebled, where there is merely chronic inflammation, and where, though the pulse is quick, it is so only from debility? Surely we should not argue, from the patient having a quick pulse, that he should be put on a lowering diet and use debilitating remedies. Is it not notorious that, on the approach of death in very old or infirm persons, the pulse beats with a very increased quickness? When the heart has little ability to send forward its contents, its efforts, of course, are more feeble, but, at the same time they are more frequently made. That quickness of the pulse, then, and febrile heat which are occasioned by ulceration of the lungs, in conjunction with chronic inflammation, cannot be cured, in the opinion of Dr. Stewart, by such means as will depress the general vigour of the system. If a patient, of an emaciated habit, with feverish symptoms, had foul ulcers on his limbs, would any one of our modern physicians prescribe blood-letting and purging as the best means of curing him? Or would he direct all his attention to the very slight inflammation at the edges of the ulcers, and neglect entirely the constitutional derangement? Would he not try rather, by a nourishing diet, mild cordials and bark, to invigorate the system, that the vessels may recover their native tone, and, instead of a vitiated secretion depending on debility, be enabled to exude a healthy lymph, which may form granulations and consolidate the diseased surface? The very same thing Dr. Stewart wishes

to effectuate in cases of internal ulceration and debility. In consumption, he gives nourishing diet, pure or diluted wine, bark, steel, and other tonics; he uses exercise on horseback or in a carriage, or in any other practicable mode; and he employs friction with vinegar and cold water; all with the design of bracing the system, and enabling it to assume that healthy action which alone can render it capable of resisting the sudden vicissitudes of our climate. In short, if he cannot make the weather suit his patient, Dr. Stewart tries to make his patient invulnerable to its attacks. He has been accused of giving wine and beef-steaks to a person who is spitting blood, and at the same time under the influence of strong arterial action. So far from this being the case, he would be amongst the foremost, in such cases, to recommend the antiphlogistic regimen, and even to administer digitalis, a medicine which he unequivocally condemns in phthisis. But even in chronic inflammation, and ulceration of the lungs, he approves of blistering, and makes no opposition to the occasional employment of topical blood-letting. He pays the utmost attention to the regular evacuation of the bowels, and at the same time, takes great care not to give heating cordials.

I regret that I cannot give a more detailed account of Dr. Stewart's practice; but I hope that it is not misrepresented. I know that the late Dr. Gregory lent all the weight of his great name to sanction it, and even carried the treatment to a far greater length than was ever done by Dr. Stewart. In the convalescence from fevers, we are every day taught that a quick pulse is no obstacle to the use of cordials. In a case of this kind, and at the moment when the patient had lost, by stool, about four quarts of blood, I prescribed wine, brandy, and laudanum, with the happiest effect.

All this may appear very trifling to some persons; but I am confident that if every physician who has been many years in practice, would candidly state the result of it, medicine would, in this way, make a much greater progress than can possibly be made by the tedious detail of innumerable insulated cases. However, I am not adducing the foregoing remarks as an example of this kind.

Sketches of Cases wherein the Essential Oil of Turpentine was administered for the Expulsion of Intestinal Worms. By James Kennedy, M. D., of Glasgow.

(From the London Medical Repository for February, 1823.)

Essential oil of turpentine, on the evidence of diversified observation and experience, may now be regarded as one of the

best and most certain means of procuring the expulsion of intestinal worms. These sketches are consigned to the repositories of practical pathology, with the object of contributing additional illustration of the virtues of a medicine so diffusive in its influences, and so efficacious by its powers.

Case 1*st.* March, 1815.—R. H., a lad in his sixteenth year, had already been two days sick and confined to bed. His disease, on examination, now displayed the chief symptoms of inflammatory fever,—pain in the frontal and temporal regions, in the course of the spinal column, and along the centre of either thigh; thirst, gastric irritation, and constipated bowels; dry mouth, furred tongue, parched skin; pulse strong and accelerated; abdomen tumefied and intolerant of pressure; eye suffused and glossy, with dilated pupil, and great impatience of light. Twenty-four ounces of blood were taken from his arm, and a strong dose of calomel and jalap prescribed.

Next morning the symptoms were aggravated. During the night the patient was hot and agitated, and unable to sleep. He had three scanty liquid stools, and was much distressed with tenesmus and headach. At three different times, imperfect convulsions supervened, and these were preceded by grinding of the teeth, muscular spasms, flushings of the face, muttering, moaning, and horror. Reflection on the assemblage of morbid signs induced me now to ascribe to the fever a sympathetic character, and to refer its development to the ascendancy of verminous irritation.

This case being deemed favourable for a trial of the anthelmintic powers of oil of turpentine, an ounce of that medicine, in a sufficient quantity of sweetened milk, was administered at noon, and another dose of the same strength, with an equal proportion of castor-oil, in a similar vehicle, at the end of four hours. About five o'clock, P.M., the patient began to experience severe gripes, with giddiness and intellectual confusion. Before six, a profuse perspiration took place, and the functions of his bowels were actively resumed. With much pain and straining, he obtained a copious discharge of liquid, scybalous, muculent* fæces, along with which, an intervolved mass, composed of twenty-one *dead* lumbrici, and a countless number of ascarides, was expelled. The largest of the former measured eleven and a half inches in length, and, at the thickest, seven-

* "C'est une erreur accréditée parmi les personnes qui, à toutes sortes de titres, ont soin des malades, que les vers peuvent être expulsés dans un état de decomposition ou de fonte, soit par l'effet des médicamens, soit même par les seules forces de la nature: cette opinion doit être laissée aux nourrices et aux garde-malades: ces prétendus vers fondus ou hachés ne sont autre chose que des mucosités intestinales légèrement coagulées, ou des fragmens des substances alimentaires filamenteuses."—Laennec.

eighths of an inch in circumference. Many of them were eight, the shortest three inches long. Five more of the first species were ejected in the evening, two during the night, and three on the following morning. Of the second, the number was very great. The worms, as well as the accompanying fæces, were strongly impregnated with a terebinthinate odour.

Half an ounce of castor-oil, in an anodyne draught, was exhibited at bed-time, and the young man enjoyed moderate rest through the night. His febrile symptoms gradually subsided; and in a few days, with the use of an occasional laxative, his health was confirmed.

Case 2*d.* September, 1815.—Mr. C., married, and in his thirtieth year, requested my advice. This person is tall, fair, and thin. His habits are temperate, but he is considerably emaciated. His complaints are attended with the usual symptoms of alvine irritation. There is a perpetual sense of cold in his abdomen, accompanied with spasmodic twitchings in the umbilical region. His respiration is heavy and fitful; the odour of his breath and perspiration sourish and fœtid. He is teased with an incessant tickling of the pharynx, occasioning a dry, hoarse, frequent cough. The mucous membrane of his nostrils is overspread with the particles of a furfuraceous exudation. His urine has a milky appearance and consistence. His pulse is hard, rapid, irregular, intermittent. Violent palpitations of the heart often fill his mind with alarm; and he is debilitated by profuse discharges of blood from the nose.

At intervals, during the greater part of his life, this man has experienced distress from the excitement of alvine worms. Almost every remedy accounted vermifuge, whether of charlatanical or scientific prescription, has been tried by him, without permanent benefit. Among others, the celebrated nostrum of Madam Nouffer, though submitted to fair and repeated experiment, was unavailing, and left him despondent of relief. He now consented to use the oil of turpentine as a last effort for health, and professed himself ready to take it, in whatever form it might be prescribed.

Conformably to this resolution, he swallowed two ounces of the medicine, in an aromatic solution of gum arabic, at the time of going to bed. Early in the morning, he awoke with oppressive headach, accompanied by giddiness, confusion of mind, and general agitation. An ounce of castor-oil, in a glassful of acidulated water, being now given to him, he replaced himself to rest. At the end of two hours, severe pains in the abdomen, which was much distended, and incapable of bearing the gentlest pressure, aroused him from an unrefreshing sleep. Forthwith, he had an enormous alvine evacuation, with the matter of which, sixteen lumbrici, and an astonishing

swarm of ascarides, *all dead*, were commingled. In another, he expelled six of the former, and many of the latter kind. During the day, he passed several loose stools; but these did not dislodge any worms. Qualmishness, at the same time, and exhaustion, with a sense of inanition, remitting headach, and frequent tendency to perspire, annoyed him; while his urinary, intestinal, and cutaneous excretions, emitted a strong terebinthine smell.

Next night he repeated the medicine in an equal dose, and on the following morning took castor-oil. His feelings and condition were, on this occasion, little different from those he experienced on the preceding day. The first evacuation was copious: it contained four large lumbrici, and great numbers of the ascarides; and all of both kinds, as formerly, were quite dead. In the course of the day, his bowels were frequently moved, without any worms being expelled. The fæcal matter, however, was muculent, slimy, dark-coloured, and gave out the odour of turpentine.

This man took small doses of castor-oil on the four subsequent days; but it was nearly a fortnight before his headach and gastric disorder altogether ceased. Since that time to the present (February, 1821), he has remained free from every symptom indicative of vermigenous disease.

Case 3d. May, 1816.—W. B., aged eighteen years, has for a long period of his life been afflicted with convulsive paroxysms, bearing great affinity to those of epilepsy. His person is emaciated; his countenance pale, languid, anxious; his eye dull and watery. Depraved appetite and constipated bowels, alternating with diarrhœa, have long distressed him. His breath is fœtid, his mouth foul, his belly tense and swollen. During sleep, he grinds his teeth and utters loud moans, is restless and agitated. These and other circumstances seem to authorize the opinion, that his convulsive movements originate from intestinal excitement produced by the presence of worms.

For the purpose of expelling these noxious animals, an ounce of oil of turpentine, suspended in three ounces of sweetened milk, was given to him at bed-time, and early next morning a repetition of the same dose. Awaking from a disturbed slumber, he experienced great oppression in the epigastric region. His mouth was dry, his breath tainted with the medicine, his thirst insatiable. Intense headach made him dull, peevish, and spiritless. Globules of ungenial perspiration glistened in his forehead; his carotid arteries pulsated with extreme, almost audible violence. Diminution of sensibility supervened, and, after some time, gave place to a convulsive paroxysm, in no degree different from those which he had heretofore sustained. This was followed by painful and repeated vomiting, and the

matter thus disgorged was deeply impregnated with the flavour of the drug. He had only one stool during the day: it emitted a similar odour. With its contents, two lumbrici and several ascarides were intermingled.

At night, being disgusted with the medicine, he declined taking it; but, in its stead, received a solution of Epsom salts, which, on the succeeding day, produced two alvine evacuations imbued with a faint terebinthine scent. No argument could induce this person to proceed farther with the treatment, and he was, in consequence, left to appreciate the suggestions of his own mind.

After the lapse of two months, application was again made to me in this young man's behalf. His disease seemed not, in the interval, to have undergone any change. The original mode of treatment was again advised; and to this, on reflection, he yielded a reluctant assent. In compliance with this advice, he took an ounce of castor-oil in the afternoon, and a purgative powder, containing calomel and gamboge, at the time of retiring to bed. Next morning he awoke about the dawn, and, on having a motion of his bowels, took one ounce of oil of turpentine in an aromatic emulsion. At the same time, the operation of his medicine was assisted by the liberal use of warm demulcent drinks.

Much unpleasant feeling, and several discouraging symptoms, but likewise some perceptible advantage, resulted from this exhibition of the remedy. Considerable nausea was excited, and headach, and thirst, and abdominal distention. Tendency to convulsions was not perceived: strangury, however, supervened, and discharges of bloody urine. Nevertheless, these soon subsided under the use of topical fomentations. The bowels acted frequently during the day, and, with a great quantity of slimy fæces, expelled many lumbrici, all dead, and imbued with the terebinthine taint. Two days subsequently, the same remedies were a second time administered, after being preceded by the warm bath. Similar phenomena, with the exception of strangury, were on this occasion elicited. The urine was nearly limpid: it had a pale yellow tinge, and, with the perspirable and fæcal excretions, retained the flavour of the medicine.

With the intervention of a few days between each trial, the same course of treatment was three other times repeated, with effects not particularly different. Numerous dead lumbrici, and many of them of a large size, were by these means dislodged; but no trace of ascarides could at any time be discovered. Up to the last-mentioned date, the young man's convulsions are not known by me to have reappeared.

Case 4th. July 30th, 1817. M. T., an active girl, seven

years of age, was this day visited by me, at her parents' request. By their account, her health had been defective during the last twelve months. Her skin, at present, is hot and dry; her pulse small, rapid (126), irregular. She has constant thirst; impaired appetite; headach; occasional hiccup; hoarse, frequent cough; muscular spasms. Sometimes she vomits her food; her bowels are often constipated, often morbidly free, often griped. A sense of coldness pervades her abdomen, particularly its umbilical region: it is distended and irritable under pressure. Oftentimes she starts in great alarm from an unrefreshing and disturbed sleep, and utters piercing shrieks. While asleep, her under lip is intermittingly agitated by tremulous motions. An evanescent blush plays upon her cheeks; her eye has a characteristic wildness of expression; its sclerotic tissue is pearly white and glossy, its pupil languidly contractible; her whole physiognomy, indeed, is indicative of disease originating from excitement of the alimentary canal.

This girl was put under a course of alterative evacuants, which, in the space of one week, brought away very great quantities of discoloured, scybalous, muculent fæces, with the effect of relaxing the abdominal tension, and mitigating the cephalalgic pain. In other respects, her pathognomonic symptoms underwent little change. The same remedies were continued during another week, with the addition of the warm bath every second evening. Their operation, at the same time, was assisted by frequent frictions of the abdominal and lumbar regions with an ammoniated liniment combining turpentine and camphor. This treatment effected the dislodgment of much sordid fæcal matter, in which were found several detached pieces of tapeworm. Each of these was living, and varied, in length, from one to three inches. Little improvement of the patient's health, however, had as yet become apparent. Tæniæ infesting the alimentary system were now regarded as the chief source of all this young person's disorders, and for their expulsion the oil of turpentine was prescribed. For this purpose, she took four drams of the medicine mixed with an ounce of syrup of roses, after her having been immersed for fifteen minutes in a warm bath. The frictions were also continued; and, at the end of six hours, half an ounce of castor-oil was exhibited.

About an hour after taking the medicine, she required to be placed in bed by reason of intense headach, vertigo, and a disposition to vomit. Anon, she unloaded her stomach by vomiting, and soon fell into a profound sleep, during which her body was covered with a profuse perspiration. When she awoke, being more composed, her mother prepared a second mixture, and made her swallow it. Effects not materially different from those induced by the first dose again supervened. The medi-

cine, however, was with much difficulty retained, and free action of the cutaneous exhalants re-established. In the afternoon she received a purgative powder, which soon began to operate. By this means, great quantities of ropy, offensive fæces were expelled; and in one of the dejections was found a piece of tænia, measuring four feet in length, and showing no signs of life. Next day the same treatment was repeated, with the most beneficial effects. In the course of three days eighteen pieces of the worm were discharged from her bowels. These pieces varied from one inch to a foot in length; they were all dead, and more or less impregnated with the drug's odour.

Cathartic powders were occasionally administered to this patient during the five subsequent weeks, when their use was discontinued. For some time her alvine discharges contained many minute portions of lifeless tape-worm; but latterly these altogether disappeared, and the patient for many months remained in a confirmed state of health.

February, 1819.—About the commencement of this month, some fragments of living tænia being detected in the dejections of the same girl, she was again placed under my care. Her health at this time was imperceptibly affected: she took food, and amused herself with all the vivacity of a sprightly child. Her complaints consisted solely of occasional gripes and unpleasant tension, without enlargement, of the abdomen. With the object of ascertaining its helminthagogue virtues, in a different mode of exhibition, I now instituted a trial of the oil of turpentine in the form of an injection. Under my own direction, accordingly, a lavement, consisting of an ounce of the drug, and six ounces of tepid milk, was slowly and cautiously introduced. During its reception, she complained of acute pain in the bowels; but this was only momentary, and unaccompanied with any other troublesome sensation. After being retained about twenty minutes, the medicine was expelled, and brought with it a copious dejection, in which eight pieces of tape-worm, all dead, and of different lengths, were discovered. At the end of another twenty minutes, some urine was voided: its colour seemed natural, but it emitted a strong terebinthine smell. In two hours more, she had a second intestinal evacuation, from the contents of which two pieces of tænia were taken. Late in the same evening, a clyster, in all respects similar to the former, was carefully injected. This was immediately thrown back, along with a small proportion of fæcal matter. It dislodged one short fragment only of the animal, and no disagreeable feelings were induced by its operation. The joints of the worm manifested no signs of life. The child afterwards took supper as usual, was put to bed, and slept soundly till morning. At an early hour five grains of calomel were given

to her, and in the course of the day produced four free stools. With the first of these, two portions of tænia, dead, and each measuring five inches, were rejected. At my suggestion, her father, as soon as he rose from bed, ascertained the following circumstances;—her linen, a slip of flannel bound round her neck, her night-cap, and the contiguous bed-clothes, gave out a distinct smell of turpentine; her breath, also, together with her urinary and alvine excretions, was deeply impregnated with the same odour. On the subsequent day two additional pieces of the worm were voided by stool: they were dead, faded, and shrivelled. For several weeks my patient occasionally took aperient medicine; and at the time of my departure from Strathearne, in 1821, experienced no relapse of the disease.

Case 5th. June, 1821.—Mrs. J., the mother of a numerous family, and far advanced in life, had suffered during several years from a variety of complaints, which medicine hitherto had proved unable to remove. Her existing symptoms were, in chief,—unequal arterial action; nocturnal paroxysms of feverishness, accompanied with general disquietude and insomnolency; excess of bile in the circulating fluids; œdematous condition of the ancles and feet; obtuse fixed pain in the left hypochondriac region, increased by pressure; a deep-seated, elastic tumour, perpendicularly oblong, and large as a new-born infant's head, situated in the parts corresponding to the sigmoid flexure of the colon; successive developments of the hæmorrhoidal excrescence, giving rise to frequent small discharges of blood; coffee-coloured, sedimentous urine; occasional strangury; fits of the senile cough determining copious expectoration of yellowish frothy mucus; distaste of aliment; interchanges of constipation and liquid dejections; flatulency, with offensive eructations, and the more common symptoms of the dyspeptic and hysterical states.

Various modifications of alterative, deobstruent, and tonic medicines, with tepid and warm bathing, abdominal and vertebral frictions, were employed during the eight subsequent months, as the means of mitigating or removing this person's complaints. Their best effects, however, produced only temporary and imperfect benefit; and relapses were, at all times, readily determined by causes apparently very little calculated to deteriorate her health.

Reiterated failure of these remedies ultimately gave rise to a suspicion that tænia, acting on the internal alvine surface, might be the source from which all my patient's constitutional derangements arose. With the design of proving this indication, therefore, and, at the same time, of forwarding her cure, it was forthwith resolved to administer oil of turpentine, and to

promote its influences by the exhibition of a powerful co-operative evacuant.

Pursuant to this view of her treatment, six drachms of the medicine, suspended by agitation in two ounces of an aromatic infusion of rhubarb, were taken by the patient, at noon and in bed; and, after an interval of three hours, the dose was repeated to the same extent. Fomentations of the abdomen were superadded, and, when another hour had elapsed, two drops of croton-oil, in a glass of sherry-wine, administered. In twenty minutes, acute intestinal spasms came on, and gradually increased. The bowels experienced a most distressing sense of distention, and an indescribable feeling was excited in the left lumbar region. By and by, a cold perspiration overspread the face and chest: tremors of the inferior extremities succeeded: ringing of the ears ensued: the features at last became cadaverous, the eyes agitated by nervous motions; and the sufferer sunk in an alarming swoon.

By the use of analeptics and stimulants, she was soon restored to consciousness, and immediately (about fifty minutes after the croton-oil was taken) the bowels began to act. The matter of three evacuations presented no unnatural appearances: it emitted, however, the true terebinthine scent. That of a fourth, which soon followed, and was attended with exquisite pain, consisted almost entirely of an innumerable swarm of very small worms, lifeless, and floating in a thick muco-gelatinous fluid, tinged with a few streaks of florid blood. This fluid measured altogether more than three English pints.

The patient now replaced herself in bed, after having received a mucilaginous anodyne draught. In the afternoon and course of the night, which passed away in unexpected tranquillity, she expelled, in three additional dejections, a great number of the same kind of worms. They were all dead, suspended in the viscid mucus, and very sensibly impregnated with the odour of turpentine.

Notwithstanding the distress induced by our first attempt, my patient hesitated not to prosecute the same system of treatment. In consequence of this resolution, she took two drachms of the oil of turpentine, in different vehicles, with which a proportion of castor oil was conjoined, on the mornings and evenings of alternate days, till the dose had been six times repeated. By the end of a week, this lady's circumstances were found to be exceedingly ameliorated. In the interval, great numbers of worms were expelled in occasional free evacuations. All her recent, and, at the same time, many of her original symptoms ultimately subsided, and the swelling in her left side no longer remained. By a varied management of her alimentary

functions, this person in the course of a few weeks regained a state of health which withstood the atmospheric vicissitudes of winter; and its tenor has never since been so disturbed as to require the assistance of medicine.

Remarks.—By the evidence of these and similar cases, it is made probable that the essential oil of turpentine possesses virtues capable of fulfilling the chief indications of treatment in disease originating from the presence of worms in the alimentary canal. The principle on which these virtues depend does not seem, however, to be altogether distinguished by the true cathartic character. This medicine, according to my apprehension, contains some inherent quality of resisting absolute decomposition by the assimilative operations of the organs of nutrition, and passes along the intestines in a great measure unchanged. There the proper terebinthine* principle, whatever that may be, comes in contact with the parasitic animals, and, by the influence of its specific properties, deprives them of life. By this means they are brought into the state of inert matter, and thereby subjected to the expulsive action of the organ, whose cavity is the place of their production, and whose functions their existence disturbs.

Each of the patients whose cases have been detailed, though in different degrees and at different intervals, after taking the medicine, experienced various unfavourable symptoms. Excitement or increase of headach, gastric disorder, nervous exaltation, convulsive movements, seem to have been induced by its action on the sentient tissues of the organs of assimilation. Strangury and emission of bloody urine were, on one occasion, an evident result of the drug's exhibition; but their presence was transient, and may, without infringement of philosophical rules, be regarded as a trivial exception to a general law. The cause, whatever it be, of the other unnatural manifestations, does not communicate effects permanently injurious to the system. It is even questionable whether these same manifestations do proceed from an internal use and agency of the medicine itself, or from the nervous exasperation produced by reaction of the disturbed animals when subjected to the influence of its specific powers.

Analogy, though at best but a trustless guide, may without unfairness be allowed to contribute the means of determining what amount of certainty is due to the foregoing surmise. For this purpose, let an ounce of the essential oil of turpentine, suspended in tepid milk, and sweetened with sugar, be given to a healthy adult; and, with careful observation, mark the result.

* Turpentine offers a subject of curious and useful research to the investigators of vegetable chemistry.—K.

This has been done several times by my particular direction, and the uniform consequence was,—the medicine, after being retained in the stomach, two hours at the longest, was rejected by vomiting. In most of the persons on whom this experiment was made, there could be no doubt of the cause of this rejection being in a chief degree dependent on the power of imagination fostered by the drug's unpleasant taste. Beyond the gastric irritation, no other symptom supervened in any of these instances; but, notwithstanding its presence in the alimentary organs was short, and its expulsion appeared in all of the cases to be complete, the turpentine had partially yielded to the effects of absorption, and its odour was perceptibly exhaled by the cutaneous, urinary, and alvine excretions.

When we have resolved on administering oil of turpentine for the dislodgement of parasitic worms, it may be requisite that, at the same time, we combine measures adapted to obviate the injurious effects which the medicine sometimes exerts in the functions of the sanguineous and nervous systems. For this purpose, my own experience enables me to prefer the use of preparatory and consecutive evacuation. In the preceding histories, when the disease was intense and obstinate, these precautionary means are represented as having failed of producing advantage: in ordinary and milder instances, however, their beneficial tendencies will be apparent. Croton-oil, from the promptitude of its operation, may yet be so managed as to become an energetic agent in completing the expulsion of the dead worms, together with the helminthagogue drug, from the system, and thus prevent the distresses imputable, in a great degree, to absorption of the terebinthine principle.

Oil of turpentine, in young and delicate subjects, may be exhibited by injection. In this form, however, it is less efficacious, although less disagreeable. It acts with more energy, in this way, on that kind of worms which infests the excreting portion of the intestinal tube. Introduced, indeed, into the alimentary canal, in whatever way, the medicine seems to determine particular effects by means of its influence on the actions of the muciparous organs: by rousing the energies of these, when overpowered or exhausted, it contributes to restore, to their salutary proportions, the different secretions by which the processes of alvine evacuation are facilitated and renewed. When disease, moreover, depends in a chief degree on embarrassment, disorder, or subversion of the intestinal functions, an appropriate exhibition of this oil will be found materially beneficial in commencing or promoting that kind of reaction in the parts, wherewith Nature invariably strives to overcome the causes from which the disturbance of their health may have originated. For this purpose, as experience appears to show, it imparts by

its proper efficacy, whatever that may be, a peculiar excitement to the nervous fibrils, vascular ramifications, mucous follicles, and muscular fibres, throughout the whole extent of the alvine surface; and by the complicated impulse thus induced, becomes a powerful auxiliary in obtaining the recovery of functional balance which was lost.

Instructed by this view of the terebinthine properties, I am at present directing their employment in a most obstinate and protracted case of that disease, to which, with little apparent propriety, the name of *diarrhœa tubularis* has been applied. Hitherto, many favourable circumstances have combined to encourage the hope of my patient's sufferings being, at last, considerably mitigated. Be the results of the experiment, however, what they may, its detail is intended at some future period to constitute a brief pathological report, having for its object, to assist in discriminating the indications under which the essential oil of turpentine can be advantageously prescribed.

More than one circumstance in the case No. 5 will be admitted to involve considerable interest. The tumour situated in the patient's left side may, without impropriety be regarded as one of the encysted kind. It had local connexion with the colon; it was ruptured by the medicine's topical effects; its contents were thrown into the channel of the intestines; and there can be little doubt of its having been replete with a particular fluid, containing a multitude of parasitic worms. All these, when expelled from the bowels, were dead; and, with the utmost care, it was found impracticable to obtain even one of them in an entire condition. Subjected to careful observation under a common microscope, several of the most perfectly retained characters which they were observed to possess, induced me to consider them as mutilated fragments of the vesicular worm described by later helminthologists* under the generic designation *ditrachyceros*, with reference to the number and roughness, or *diceras*, to the number only, of its horns.

Notwithstanding my own mind felt satisfied of the identity between these and the rough double-horned worm originally discovered and described by Sultzer, and subsequently by Lesauvages, yet, from the necessarily imperfect nature of my observations on the disorganized remains of the animals, I do not find myself warranted in advancing this report as other than

* Sultzer: Dissertation sur un Ver Intestinel, nouvellement découvert et décrit sous le nom de Bicorne Rude, *Strasbourg*, 1801.—Zeder: Anleitung zur Naturgeschichte der Eingeweidewürmer, *Bamberg*, 1803.—Laennec: Mémoire sur les Vers Vésiculaires, *à Paris*, 1804.—Rudolphi: Entozoörum, sive Vermium Intestinalium Historia Naturalis, 3 vols. 8vo. *Amstelodami*, 1810.—H. Cloquet: Dictionnaire des Sciences Médicales, Tome XXII. *à Paris*, 1818.—Lesauvages: Bulletins de la Société de la Faculté de Médecine, Tome VI. p. 115.

probable testimony to the facts in natural history which these philosophers had heretofore promulgated. As the subject, however, embraces matter alike amusing and instructive, the readers of the REPOSITORY may take interest in contemplating its chief features in the form of a descriptive sketch.

History.—Dr. Sultzer's patient was a female, aged twenty-three years. From infancy, this person had been subject to frequent swoonings, and, since her seventeenth year, to a diversity of nervous affections. In her twenty-second year, these symptoms became very distressing; and, superadded to them, were an habitual languor and inappetency, obtuse colic movements, and a fixed pain in the left hypochondrium, which pressure or motion readily aggravated. Against these morbid indications she took a large dose of the family panacea,* a drastic quack medicine, with the effect of determining a "*superpurgation*" which continued nine days. This was accompanied with vomitings, violent cramps, and "colics so frightful," as to excite suspicions of her being poisoned. Subsequently to this occurrence, the "coliques soundes" and pain in her left hypochondrium never altogether subsided.

Some months after this, she had inflammation of the throat; and, as a remedy, swallowed a draught containing manna and sulphate of soda. This purgative, in the course of two days, procured the discharge of a "prodigious number of *bicornes rudes.*" Among all these, four only were found entire: the rest wanted their horns or external membranes; these parts, however, were detected separately in the fæcal matter. From this time her left hypochondriac region was quite free of pain, except when strongly compressed: by and by, uneasiness there entirely ceased, and the patient perfectly recovered.

Dr. Sultzer regards the whole tract of the intestines as the "habitat" of these animals. From analogy of their relations to other "vesicular worms," however, Dr. Laennec considers them as having been included in a cyst, which, by its ultimate rupture, threw them into the cavity of the alvine tube. This sentiment is founded chiefly on the pain which habitually existed in the left hypochondrium during the existence of the worms, and was probably caused by their presence, they having never reappeared after their expulsion. This "fixity" of the pain seems to him to indicate their confinement in a particular spot from which they could not escape; for worms, such as tæniæ and lumbricoid ascarides, which freely pervade all parts of the intestinal canal, carry signs of their presence into every place they successively occupy.

Description.—Ditrachyceros constitutes a genus of vesicular

* La poudre d'Ailhaud.

worms: of this, one species only has hitherto been observed by naturalists. It is the *ditrachyceros rudis* of Sultzer, the *cysticercus bicornis* of Zeder, the *diceras rudé* of Rudolphi, and, in his native idiom, the *bicorne rudé* of the Strasburg Physician by whom it was first discovered.

This worm is of a fallow (*couleur fauve*) colour, and, including all parts, about four inches long. Its body is composed—1st, of an exterior, thin, floating membrane, enveloping it on all sides, without adhering to it, except in the proximity of its horns; 2d, of a stronger, thicker membrane, which is also attached to the roots of its horns; 3d, and of a sort of cyst or bag, smaller than the two former, and included in the cavity of the second. Each of its horns is thick as a horse's hair: both of them are conic, rugous, and somewhat flattened on the sides of their base, where they unite and form a sort of common trunk, which is very short, and moves in all directions as on a pivot. Examined with a microscope, the horns appear formed of a homogeneous substance, pitted with cells which are larger as their site is nearer the base (*pedoncle.*) They are crossed longitudinally by a sort of line, consisting of brittle matter. Their surface is bristled with numerous pyramidal flakes.

The cavity of the body contains a limpid fluid, and its investing membrane, seen through a microscope, appears all over, both internally and externally, bestudded with tubercles, varying much in form, being oval, round, triangular, or trapezoidal, indented in their edges, and separated from each other by irregular depressions. The internal cyst is dark brown: both its surfaces are marked with deep wrinkles, but it contains no aperture, and is contracted into a point towards the back, where it is connected by adhesion to the inner face of its envelope.

Researches on the Pathological Anatomy of the Digestive Canal, considered in its Sub-diaphragmatic Portion. By M. Andral, Jun. M.D.

(From the London Medical Repository for March and April, 1823.)

The following memoir was read at the *Académie Royale de Médecine*, in the sittings of March and April, 1822, and received the highest commendation from Messieurs Chaussier and Clocquet, who were deputed to render some account of it to that learned body. To us the pathological observations contained in it appear to be possessed of the highest interest, and seem evidently written by one who must have paid very considerable attention to the anatomy of the body under health as well as

under disease. We, therefore, consider it richly to deserve a place in the REPOSITORY.

ART. 1.—*Anatomical characters of inflammation of the digestive tube.*

The digestive canal, inflamed, when viewed externally, is generally contracted, and appears injected. Contraction of the intestines, however, sometimes exists independently of inflammation. It is not unfrequent, for example, to find the pyloric portion of the stomach strongly contracted and reduced to the size of a small intestine, although no mark of inflammation can be discovered on its internal surface: we have also frequently seen the great intestine strongly and universally contracted from the cæcum to the rectum, and yet no derangement in its mucous coat. The injection, when seen externally, is sometimes seated in the sub-peritoneal tissue only, more commonly it has its seat in the cellular layer, situated between the mucous and muscular coats, but in no case can the external appearance show the state of the mucous coat. It has frequently happened to us, to find this membrane evidently inflamed, disorganized, and ulcerated, in portions of the intestines, which, when seen and examined externally, had been regarded as healthy. An important error may therefore be committed, if, as is sometimes done, we pretend to judge of the healthy or morbid condition of the intestine by the appearance of its external surface.

If we examine its internal surface, we find it present a multitude of different aspects: an uniform red colour is often observed of a greater or less extent, which varies from the most intense vermillion to the deepest brown. Sometimes this redness loses itself by little and little, as is observed more particularly in the small intestine; whilst at other times it terminates abruptly, as is frequently observed at the union of the pyloric portion of the stomach with the splenic, at the junction of the stomach and duodenum, and at the ileo-cæcale valve, the superior surface of which is often found very red, whilst the inferior is very white, and *vice versa*. The valvulæ of the small intestine are commonly of a much deeper red than the intervals which separate them; but if they are unfolded, this more intense colour is observed to disappear: at other times there exist, in several places, red or brown patches of a round or irregular figure, between which the internal surface of the intestine is nearly white: these patches seem to form so many isolated phlegmasiæ. It is not uncommon, in place of an uniform red tint, to meet with numerous arborescent appearances, occasioned by a strong injection of the vessels, and their numerous anastomoses; around these vessels a crowd of small red points frequently appear, which seem to be sometimes owing to a more violent partial injection, and occasionally to a slight circum-

scribed sanguineous extravasation. Finally, pimples, pustules, and fungous excrescences of different forms and sizes, sometimes arise from the internal surface of the intestine.

It is not sufficient, however, to have opened the intestinal canal, and to have observed, with more or less attention, the state of the internal surface: the mucous surface must be separated from the subjacent tissues; must be studied when thus separated; and the lesions which the other tunics have undergone, be afterwards investigated. This is the manner in which we shall proceed at present.

If we open the stomach, or any portion of the intestine of a living animal, we find that, except during digestion, the mucous membrane is every where of a white or rosy white colour; through its tissue, which is perfectly transparent, the subjacent laminated tissue may be discerned: it is commonly but triflingly injected; but it becomes more so if the animal struggles much. During the process of chymification, the mucous coat of the stomach is commonly of a deepish red colour: in proportion as the chyme passes forward in the intestines, their mucous coat becomes vascular like that of the stomach. The same colour is observed in the portions of the great intestine, where the fecal matters accumulate and sojourn. Thus, in several rabbits where we have had occasion to examine the digestive tube, we have found the mucous coat of their enormous cæcum, which is always filled with a great quantity of fæces, of a lively red.

With regard to thickness, the mucous membrane of the digestive passages presents great varieties in its different parts. In the stomach it is at its *maximum;* it is thinner in the small intestines; and still more so in the cæcum and colon, where it only forms a sort of very thin cuticle.

Its consistence is every where in a direct ratio to its thickness: thus, in the stomach it can easily be detached in large flaps, and may be pulled in different directions without tearing, whilst in the great intestine it tears on the least effort.

Its adhesion to the subjacent laminated tissue is by no means considerable in the stomach. In the duodenum it can only be separated with great difficulty: in the remainder of the small intestines, the adhesion of the mucous coat is more intimate where it covers the valvulæ, than in the intervals between them; in the great intestine this adhesion is tolerably strong.

Such has appeared to us the mucous membrane of the digestive canal in its physiological state. When attacked with inflammation, it presents numerous modifications in its colour, thickness, consistence, texture, and frequently in the state of its follicles: the liquids which cover its surface, also undergo remarkable changes with respect to quantity and quality.

The first effect of inflammation is to occasion a greater flow of blood into the part of the mucous coat which is attacked by it: when detached under such circumstances, and held between the eye and the light, this membrane sometimes appears overrun by numerous vascular networks; whilst at other times it presents an uniform red tint, and completely intercepts the passage of the luminous rays. This latter condition indicates a more considerable afflux of blood: the smallest vascular ramifications are filled with it, and are so closely compacted that no interval exists between them. Anatomists, in their artificial injections can easily give this uniform red tint to the tissues, and especially to those which are membraneous. In place of presenting a red colour, the mucous membrane, when in an inflamed state, is frequently of a *more or less deep brown colour.* This tint does not depend upon the duration of the inflammation: it is sometimes observed to supervene in a very short space of time when the inflammation is violent, at which times it seems to announce the commencement of the disorganization of the membrane. Carry into the stomach of an animal some strong corrosive poison, such as corrosive sublimate, arsenic, sugar of lead, &c. and you will find in a very short space of time, an hour for example, the mucous membrane of a cherry red in several parts, whilst in others it is of a brownish gray. Sometimes this latter colour exists alone. Professor Orfila found that the mucous coat of the stomach was of a very deep reddish brown, in animals which had swallowed euphorbium about twenty-four hours previously. In man, the mucous coat of the stomach frequently presents the same tint, whilst at the same time it is softened.

It results from these facts, that the brown colour of the mucous membrane of the digestive canal is a product of its inflammation, and that it may equally manifest itself, in violent inflammations, which occasion, in a very short space of time, the disorganization of the tissue with which they are placed in contact, and in chronic, which in process of time, produce the same result.

At the same time that the mucous coat becomes injected with blood, it must be necessarily thickened. This thickening, like the inflammation which gave it birth, may be general or circumscribed. It is particularly well marked in chronic inflammations: we have oftentimes seen, in cases of chronic diarrhœa, the mucous membrane of the colon of a thickness equal to that of the four tunics united, in their ordinary state.

The circumscribed thickening of the mucous coat is tolerably frequent: it appears under the form of round or oblong patches, projecting above the remainder of the internal surface of the canal, to the extent of two or three lines. The surface of these

patches is glossy and rugous: the mucous membrane which surrounds them is sometimes perfectly white and transparent, sometimes more or less strongly injected. Their dimensions are variable: the greatest which we have seen, were at least of the size of a crown piece; the smallest were about the size of a shilling. These partial thickenings of the mucous coat are very rare in the stomach; more common in the great intestine, especially in the transverse colon, and more common still in the inferior part of the small intestine. Sometimes one only of a greater or smaller size is met with: at other times they are found to exist in very great numbers. With respect to colour, two species may be admitted: some are red, and appear to be of recent formation: whilst others are of a white, more faded than the rest of the mucous coat, and appear to be the result of an old inflammation, the other signs of which have disappeared: it is a sort of termination by induration.

When the thickening of the mucous tunic is considerable, it preserves, in the greater number of cases, its consistence, or even acquires a greater density; but at other times we find this membrane of its ordinary thickness, and singularly softened.

Pathological observations and experiments made on living animals, show that softening of the mucous coat may take place in a very short time, provided the inflammation be very violent. Half an hour after some grains of corrosive sublimate had been injected into the stomach of a dog, Mr. Brodie found that the mucous coat of the organ had undergone a remarkable softening.

In this condition it is impossible to detach the mucous tunic under the form of membrane; it is demi-liquid, and the slightest rubbing with the scalpel, or friction with the finger, reduces it into a sort of pulp or reddish *bouillie*.

A similar softening is sometimes observed in mucous membranes, which are perfectly white. Must not this condition, like the white circumscribed patches of which we have already spoken, be considered as the sequel of an old inflammation? We shall see farther on that this pulpy sort of condition of the mucous coat, with whiteness of its tissue, is one of the lesions which the great intestine presents, in cases of chronic diarrhœa.

Not only does the mucous coat, when in a state of inflammation, become thickened and softened, but *vegetations* sometimes arise from its internal surface, of a red or brown colour, concave form, and of extreme softness, pressed one against the other, and projecting four or five lines above the rest of the membrane. They somewhat resemble the papillæ of the under surface of the tongue, supposing those divided in small fragments in the direction of their length; and, like them, they are loose and moveable. A great number of similar vegetations were found in

the stomach of a man, who died two months after having taken powdered cantharides (Orfila). We have observed them in the large intestine only.

In place of these vegetations, the mucous membrane sometimes presents white conical *pustules*, projecting from half a line to a line, being about as large at the base as a lentil, the majority depressed at the summit, and exactly resembling variolous pustules. They are rarely found isolated, being most commonly grouped, like the eruption of confluent small-pox. Between them the mucous membrane is sometimes red, sometimes scarcely injected. We have never met with these pustules in the stomach. Once we have observed the internal surface of the duodenum covered with them in its upper portion; we have seen them three times in the transverse colon; but it is in the two lower fifths of the small intestine that they most commonly show themselves.

We have more frequently witnessed in the colon, pustules of a different aspect. They are conical like those just mentioned, and are situated in the mucous coat, which, when removed, takes them along with it. Their base is, however, much broader, their height more considerable; they terminate in a pointed head; they are of a deep cherry red, and the mucous coat around them is but little injected. We cannot give a more exact idea than by comparing them to those small tumours of the skin, known under the name of furuncles or boils.

M. Lerminier has proposed to designate these different species of eruptions under the generic name of *exanthema internum* (*exanthème interne*). This is one of the lesions which the intestines present in fevers, as we shall elsewhere show.

The mucous coat, when inflamed, is modified in its functions, like other tissues, when attacked with inflammation. The examination of the changes which the constant absorption undergoes, that takes place at its surface, does not belong to the province of pathological anatomy; we shall not, therefore, enter into it here, but proceed to point out the modifications experienced by the liquids secreted by it, both as regards their quality and their quantity.

The changes in the quality of the *gastric and intestinal mucus* cannot always be easily appreciated, in consequence of its either being mixed with the drink and chyme, or with the biliary and pancreatic fluids.

It presents two important modifications: sometimes, instead of being viscid, ropy, tolerably consistent, it becomes much more liquid, and resembles serum; at other times, on the contrary, it becomes more plastic, concretes, and is transformed into a *false membrane*. All authors have spoken of false membranes lining the internal surface of the colon in dysentery, and

being evacuated per anum. May they not, in some cases, become organized, adhere together like the false membranes of the serous coats, and occasion the obliteration of a part of the intestine? We read, in truth, in authors, of examples of inflamed intestines which were by that means partially obliterated. We have never had an opportunity of witnessing the organization of this species of false membranes. We have never observed these membranes except in the stomach and great intestine.

We have found the whole of the internal surface of the pyloric portion of the stomach lined by a greyish, tenacious layer, which might be torn into flaps, and simply lying on the mucous coat, of which it was, at least, the thickness In a young girl, twelve years of age, the internal surface of whose stomach presented a great number of red patches, extending in the form of broad stripes from the cardia to the pylorus, we found each of these patches covered by a membraniform, greyish, albuminous layer: between them the internal surface of the stomach, which was very white, was not covered with any false membrane.

It is not uncommon to find the portions of an inflamed intestine filled with a *reddish liquid*, which appears to be owing to a mixture of mucus and blood. The cæcum and the end of the small intestine are the parts where it is most commonly met with. We have sometimes observed bundles of ascarides situated in this bloody liquor, which existed in no place except surrounding them.

On injecting corrosive sublimate into the subcutaneous cellular tissue of an animal, Dr. Smith found the mucous coat of the stomach covered with an abundant *sanguineous exhalation.*

The *quantity of liquid* exhaled by the mucous coat, in a state of inflammation, is sometimes prodigious. Morgagni has cited the example of a woman, who, in one day only, passed, *per anum*, forty pounds of a limpid fluid. The debility into which such a considerable discharge throws the animal economy, may be sufficiently considerable to occasion death in a very short space of time.

It is not only the secretion of the mucous coat which in this case is modified and augmented. Dissection demonstrates that the inflamed intestine solicits a more abundant *secretion of bile*, which flows towards the parts labouring under inflammation. Stoll has noticed this fact: but far from regarding the afflux of bile as being owing to a pre-existent inflammation of the intestine, he regarded the latter as an effect of the presence of the hepatic fluid.

The *submucous cellular tissue*, in the greatest number of cases, remains untouched beneath the mucous coat, when the latter is violently inflamed and even disorganized. Occasionally, how-

ever, it is found pretty strongly injected; sometimes, also, like the cellular tissue which surrounds the arteries, it acquires an extreme fragility. We may then very easily detach the mucous coat without tearing it, to the extent of several inches: in some cases sanguineous effusion takes place between its interstices (*dans ses mailles*), some examples of which we have met with in the human subject. M. Orfila observed it in animals poisoned by the daphne laureola. Finally, in many chronic inflammations, the submucous cellular tissue becomes more dense, and of a schirrous hardness; at such times, its thickness exceeds that of all the tunics united. More rarely we have found it softened and of a pulpy nature.

The *muscular coat* presents few alterations in its texture. We have, however, seen it sometimes very much softened, the slightest contraction at such times being sufficient to tear it. In other cases it has appeared to us considerably thickened. We have found it in the transverse and descending colon of a scirrhous density and hardness; but the disposition of its fibres was still very distinct, the longitudinal and circular arrangement being perfectly perceptible. Finally, we have sometimes met with it of a deep red, whilst in its sound state it is white.

But if inflammation of the mucous coat is rarely propagated to the muscular, we frequently observe the latter sympathetically irritated, and contracted in parts where the stimulus is applied. This fact may be readily proved by direct experiments: if we inject an irritating substance into any portion of the intestinal canal of an animal, the intestine is observed to become indurated and to contract, at the part where its internal surface is in contact with the poison. The contraction of the intestine continues after death, as we have already seen: being commonly partial, like the inflammation, it very rarely exists in the whole extent of the canal. One of the most remarkable facts of this kind is related by M. Tartra in his thesis:—An individual died, three months after having swallowed nitric acid; the digestive canal was reduced to so small a volume, *that it might* (as it were,) *have been held in the hollow of the hand*, its calibre, through the whole length, not exceeding, in size, the barrel of a quill.

May not the irregular and multiplied contractions, of which the muscular membrane of the inflamed intestine becomes the seat, be regarded as one of the causes of *invaginations of the intestines?* Peyer has well described the mechanism of these invaginations, from observation on living frogs. By irritating the intestines of these animals, he saw them contract forcibly in several parts, whilst in others they remained distended with aliment and flatus; these latter portions he saw receive the former. "Hæ ità ampliatæ intrà se receperunt constrictas intestini

portiones, easque sinu suo absconditas aliquam diù detinuerunt, donec fibris se denuò exporrigentibus intestini pars una è latibulo alterius, velut è domunculo limax, pristinam in sedem rediret."

But although several authors have remarked that they have constantly found the mucous coat very much inflamed at the part where *intus-susceptions* existed, and have regarded them either as the cause or the effect of such inflammation, we can affirm that they more frequently exist without inflammation of the intestines. It is probable that many of them are found only at the moment of death, a period at which the peristaltic motion of the intestines becomes much more active, as experiments, made on living animals, demonstrate.

Considered with relation to their seat, intestinal intus-susceptions are far from being equally frequent in the different portions of the digestive tube. The ileum is the most common situation in which they occur. Fabricius Hildanus and Bartholin have seen the cæcum received into the ileum; Hartmann, on the contrary, found the ileum received into the cæcum. Invaginations of the colon are very rare. Meckel says he has seen the transverse and descending colon introduced into its sigmoid flexure. Bonetus has related a case of invagination of the rectum in a man who died of obstinate obstruction of the bowels, accompanied with vomiting of a fæcal matter. Can we believe the assertion of a German writer, who says he has seen the duodenum invaginated in the ductus choledochus?

With regard to their disposition, the superior portion of the intestine is most commonly received into the inferior. Sometimes they are very numerous: we have observed as many as seven in the same subject. Their length may vary from some inches to upwards of two feet.

The symptoms occasioned by invagination of the intestines vary according to their size, situation, number, and degree of obliteration of the intestine.

The *subperitoneal cellular tissue* is still more rarely affected than the submucous: like the latter, it may become very fragile; like it, also, it may acquire considerable thickness. In the latter case, the peritoneal covering is found to be separated from the muscular coat by a cellular layer, two or three lines in thickness, whilst, in the ordinary state, we rather suppose it, as it cannot really be seen.

The *peritoneal coat* almost always remains untouched: occasionally, however, like the two laminated membranes (*comme les deux membranes lamineuses*) it is very fragile. We have found, in some cases, all the coats of the intestine equally softened at the same time, and to a very high degree. The slightest traction was sufficient at such times to tear its parietes; and

when gently rubbed between the fingers, they became reduced into pulp.

This general softening has seemed to us to be more common in the stomach than in any other portion of the intestine.

The mucous coat of the digestive canal, disorganized by inflammation, is apt to become destroyed to a greater or less extent, and whether it is, that the particles composing its tissue are absorbed, or that they are carried away along with the contents of the intestine, *ulcerations* are the consequence, which always originate in the mucous membrane, primitively or secondarily inflamed.

Ulcerations may occur in all parts of the intestine from the cardia to the anus; but they are by no means of equal frequency in every portion. Thus they are sufficiently rare in the stomach, and still more uncommon in the duodenum and jejunum. They are very common in the lower third of the small intestine, and less so in the different portions of the great intestine. We may judge of their respective frequency by the following table, which was formed from the results of fifty-three dissections, several of which had ulcerations, at the same time, in many parts of the digestive tube:—

Seat of the Ulcerations.		*Number of Subjects.*
Stomach		9
Duodenum		1
Jejunum		9
Ileum (lower portion)		26
Cæcum		10
Colon	Ascending	4
	Transverse	11
	Descending	3
Rectum		1

These ulcerations are commonly numerous, except in the stomach, where we generally observe only one or two. In the superior portion of the small intestine, they are constantly separated by great intervals. In its inferior portion, we always find them much nearer to each other: they are frequently united and confounded near the ileo-cæcal valve, so as to form, by their union, one large ulcer. In the cæcum it is very unusual to find them so confluent, as well as in the remainder of the large intestine, where the spaces which separate them are commonly more considerable than the spaces, even, which they occupy.

They frequently arise in the middle of the red circumscribed patches of which we have already spoken, and at such times the mucous coat which surrounds them remains white, as around

the patches. It is important to remark this disposition, because the perfect whiteness which exists around many ulcerations, had occasioned it to be admitted that they might occur in the mucous membrane without previous inflammation; but it is clear that they are, in this case, the result of partial inflammation.

Ulcerations also follow the pimples and pustules with which the mucous membrane is sometimes covered. If, in fact, we observe attentively these different exanthemata, a slight erosion may be perceived at the tops of many: the summit of some others will be found to have undergone a more considerable loss of substance: the small ulcer which results extends progressively from the top of the pustule to its base, finishing by entirely destroying it. Does not the greatest analogy exist between the mode of the production of these ulcerations, and the development of certain ulcers of the mouth, which likewise owe their origin to small pustules known under the name of aphthæ?

Sometimes, also, the ulcerations are, as we shall hereafter establish, the result of the separation of eschars on the mucous membrane.

Is it in the mucous follicles that ulcerations most commonly commence? Several authors have thought so. The greater vital activity in the follicles than in the other parts of the mucous coat, the more considerable vessels which they receive, the frequently extraordinary secretion of which they become the seat in many intestinal phlegmasiæ, may lead us to suppose, that whenever the mucous coat is inflamed, they are most especially irritated, become disorganized, destroyed, and ulcerated: but no fact shows that this is *actually* the case. The greater frequency of ulcerations in the lower portion of the small intestine is, in truth, in a direct ratio with the greater number of follicles in this same part; but in the duodenum, the follicles are likewise extremely numerous; they are larger, more apparent than any where else, and, notwithstanding this, the duodenum is the part of the *digestive* tube where ulcerations most rarely show themselves.

Nothing fixed can be established regarding the period at which the mucous membrane, when inflamed, becomes ulcerated: it frequently happens that no evidence of *solution of continuity* is to be met with, although it has been the seat of very old and tolerably intense inflammation. The facility and rapidity of its ulceration appear connected with some individual, inexplicable, disposition. This is demonstrated by experiments made on living animals of the same age and strength, which have been poisoned by an equal dose of corrosive sublimate. At the end of forty-eight hours, we find, in some, only a lively red colour of the mucous coat of the stomach, whilst, in others,

the internal surface of the stomach always presents one or more ulcerations. We read in Morgagni, the case of an individual who, whilst in perfect health, was suddenly seized with most excruciating pain at the epigastrium, and with every other symptom of gastritis. He died at the end of twenty-four hours: ulcerations existed in his stomach.

The size of intestinal ulcerations is infinitely varied: some are so small they will scarcely admit the head of an ordinary pin; others are several inches in diameter in every direction: we have sometimes seen them occupy the whole circumference of a portion of the intestine. We have found the mucous coat entirely removed for more than six fingers' breadth above the cæcum: it is in this part of the intestine and in the stomach that we have met with the largest ulcerations. Some are oblong, and have their largest diameter in the direction of the length, or of the breadth of the intestine; others are exactly circular, and others again are linear.

Their edges are always formed by the mucous membrane. Sometimes they are red, thick, and elevated two or three lines above the base of the ulcer, whilst, at others, they are white, thin, and on a level with the bottom. Occasionally they are irregularly formed, and present numerous fringes, which advance from the circumference towards the centre of the ulcer.

We have often met with the mucous membrane separated to a pretty considerable extent around the ulcers: when they are in great number, and in proximity with each other, the mucous coat which separates them is sometimes entirely, or almost entirely detached from the subjacent tissue. This separation, owing to the morbid alteration of the cellular tissue, is similar to that which is observed around many cutaneous ulcers.

The base of the ulcerations differs according to the time at which we examine it. If the solution of continuity be recent, the laminated tissue which forms its base is thin and white as in its natural state:—it may preserve this appearance for a longer or shorter time: but when the ulceration has already existed for some time, it commonly acquires a considerable thickness, which may be easily felt on touching the external surface of the intestine; it becomes rugous, unequal, granulated; it is of a grey, red, or brown colour—it secretes a liquid which shows only these different tints, which sometimes concretes into a false membrane, and forms, at its surface, a layer more or less dense—in some circumstances it assumes a totally black colour, and seems to be transformed into a true eschar. But most commonly the laminated tissue becomes insensibly destroyed, after the manner of parts affected with that species of inflammation, which Hunter designated under the name of ulcerative inflammation, and the base of the ulceration is then formed by the muscular

membrane. This latter coat sometimes preserves its natural appearance; at others, it becomes progressively very red, soft, black, and destroyed in its turn, leaving the peritoneal membrane exposed. The base of the vast ulcerations at the end of the small intestine, or in the cæcum, frequently show these different coats exposed in different parts of its extent. In some cases, we may in a manner follow with the eye the successive destruction of the coats from the internal towards the external surface of the intestine, and from the centre of the ulceration towards its circumference. At such times, the edges of the ulcers are observed to present, as it were, different stages. The first, the most remote from the centre, is formed by the thin or thickened mucous coat, of a red or white colour; the second, situated more internally, is formed by the laminated tissue; the third, still nearer the centre, is formed of the muscular coat; and, finally, at the base, the thin and transparent peritoneal coat is observable. This last membrane grows altered in its turn: it becomes more fragile, tears, and perforation of the intestine is the result.

Such is the progressive march pursued by these ulcerations, when they extend in depth, but most commonly they seem to have a tendency to extend in width only, at the expense of the mucous membrane alone. Thus, in the majority of cases, we have found the base occupied by the laminated tissue.

As the ulcerations may form very rapidly, so may they extend in depth with a rapidity sometimes terrific. This is attested by various cases of poisoning, in which the intestines have been found perforated in a very short time.

Under some circumstances, the perforation is effectuated from the exterior to the interior. This occasionally occurs in cases where tubercles are formed in the peritoneal coat: becoming soft, this accidental tissue destroys the serous coat, and an ulcer results, the base of which is formed by the muscular tunic. Sooner or later this becomes destroyed likewise, and the parietes of the intestines are formed, under such circumstances, at these parts of the thin and transparent mucous membrane alone: finally, this last gives way in its turn. We have recently witnessed these different stages in a young man affected with tubercular peritonitis (*péritonite tuberculeuse*).

Intestinal perforations may likewise supervene without previous ulceration, in cases where all the coats which constitute the parietes of the intestine are softened at the same time. The least force exercised upon these parietes by solid, liquid, or gaseous bodies, is, under such circumstances, sufficient to occasion their rupture. Can an intestine, the parietes of which are neither ulcerated nor softened, be so much distended with flatus or liquids as to burst? The stomach of herbivorous animals

presents this phenomenon. Frank cites facts which appear to demonstrate that a similar rupture may be produced in man by the accumulation of flatus in a circumscribed portion of the intestines. It is thus that, according to Stoll, the bladder may burst when it is distended by a too great quantity of urine.

May not intestinal worms, in some cases, destroy and pierce the parietes of the canal which they inhabit? or, in cases where many of these animals have been found in the peritoneum, have they passed through a perforation which had been already made? Some observations of Wepfer tend to cause us to admit the possibility of the perforation of the intestine by worms. In the thirteenth chapter of his treatise on the *Cicuta Aquatica*, he observes that, on dissecting dogs, he found lumbrici in their intestines, many of which were still living, and adhering strongly to the parietes of the intestine by one of their extremities, in the manner of a leech: "Proboscis firmissimè internæ intestinorum tunicæ infigebatur, à quâ etiam, sublato intestino, hirudinum instar, pendebant. Hi monstrarunt rationem et modam quo intestina, umbilicum, et inguina perforent."

Whatever may have been the mechanical or vital cause under the influence of which the intestine has been perforated, its cavity communicates either with the exterior—with some viscus—or with the cavity of the peritoneum. Hence arises the very great difference between the symptoms, which follow the perforation and which announce it.

1. *Communication between the digestive tube and the exterior.*—Examples of this are numerous: it is the artificial anus. Adhesions are established between the edge of the perforated intestine and the abdominal parietes, and no effusion can take place into the peritoneum. We can thus explain how a ball may traverse the abdominal parietes, and be voided by stool, without having been attended with fatal effects, (*Ephem. Nat. Cur.*): how a knife swallowed was discharged, at the end of seven weeks, through an abscess formed at the epigastrium, without the patient succumbing. The following case we observed at *La Charité:*—A man, thirty-four years of age, had been affected with diarrhœa for several months; a hectic fever insensibly consumed him; he complained of a violent pain in the right iliac region; this part soon grew tumefied, and fluctuation became manifest. A lancet was passed into the centre of the tumour; this gave issue to some fetid gas, with a few drops of a greyish liquid equally fetid. The following days a larger quantity was discharged; the patient soon fell into the last stage of marasmus. We found beneath the skin of the iliac fossa a vast abscess, containing a greyish liquid of a gangrenous fetor, and circumscribed on every side by thick cellular bands (*brides*). Scarcely any traces of the muscular fibres of the pa-

rietes of the abdomen were recognizable; they were blackish, soft, and, as it were, diffluent. At the bottom of the abscess was the cæcum, the external paries of which presented a perforation with irregular edges, sufficiently large to admit the extremity of the little finger. Numerous ulcerations existed in the intestine.

2. *Communication between the digestive tube and another organ.*—In this case, as in the preceding, adhesions, established by the beneficence of nature, prevent any effusion into the peritoneum. Sometimes the intestine opens into a hollow part, into which it empties itself: thus, Frank has seen a communication established between the arch of the colon and the bladder. Sometimes the dense and solid tissue of a viscus supplies the destroyed parietes of the stomach or intestines. Thus we have often seen the bottom of carcinomatous ulcers of the stomach formed by the liver or the pancreas. Frank relates cases in which the paries of the intestine was supplied by the spleen and by the kidney.

Authors worthy of credit even assure us that they have seen communications formed between the stomach and thoracic cavity. Willis, quoted by Bonetus, found in a subject the diaphragm destroyed, and the stomach opening, by a large aperture at its superior edge, into a pleuritic pouch of the right side. Van Swieten relates a nearly similar example.

3. *Communication between the digestive tube and the peritoneal cavity.*—In this case, the matters discharged from the perforated intestine are effused into the serous membrane. The peritonitis which is the consequence is attended with great variety, as regards its march, its duration, and its symptoms.

Most commonly it has a very acute progress; the patients sometimes dying a few hours after the first symptoms have manifested themselves. Some patients are warned by a very particular sensation at the precise moment at which the effusion takes place. A man, whose cancerous stomach gave way during violent efforts of vomiting, informed us that he had distinctly felt as if a ball had descended from the epigastrium towards the umbilicus, immediately before the pains of peritonitis declared themselves. Stoll relates the case of a patient who, when labouring under retention of urine, felt something suddenly give way in his abdomen, and became at the same time stiff and livid: it was the bladder which had given way. The patients experience excruciating pain; they are in a state of extreme anxiety; the features of the face become rapidly discomposed; the pulse is irregular, thready, &c.

This *ensemble* of frightful symptoms is seen to manifest itself suddenly in individuals who, some minutes before, appeared to enjoy perfect health: their digestive tube has been perforated.

These facts prove with what astonishing rapidity ulcerative inflammation may be propagated: through it the slightest enteritis or gastritis may suddenly become mortal. The following case presents a striking example of it:—

A brushmaker, aged seventeen years and a half, of a lymphatico-sanguine temperament, had always experienced very good health. On the 13th of October, 1822, at seven o'clock in the evening, he felt, without any known cause, stupor and general indisposition. During the whole of the night his skin was of a burning heat. The next day, 14th, same state: anorexia, only one stool; abundant perspiration during the night. 15th, he was received into *La Charité*. He perspired again in the nights of the 15th and 16th. At the visit of the 16th, he presented the following symptoms:—Face red; eyes sparkling; pains in the limbs; tongue covered with a thick yellowish coat; lips red; *fœtor oris*—anorexia; slight thirst; belly soft and without pain; no stool for twenty-four hours; pulse frequent, tolerably full; skin moist. (*Barley-water with gum, a lavement of marsh-mallows.*) The patient had only one dejection until the next morning.

17th. Six grains of ipecacuanha were administered: the patient vomited twice a tolerably large quantity of bile; no motion; in the night slept well; awoke in a slight perspiration.

The next day, in the morning, (18th,) the yellowish coating of the tongue had disappeared, and it was of a fine vermillion colour; the bad taste in the mouth no longer existed; the pulse was a little frequent; the temperature of the skin nearly natural.

From the 19th to the 23d a slight febrile movement continued: anorexia; same state of tongue. One motion each day after the *lavement*. (*Emollient tisannes; broth.*)

23d. The tongue was much redder; the frequency of pulse had considerably augmented; the skin was burning; the abdomen painful on pressure. Two liquid motions in the last twenty-four hours. This return of the symptoms was combated by the application of eight leeches to the anus. (*Barley water diet.*)

During the day, the pains of the abdomen assumed a terrifying intensity, and in the night he began to vomit a great quantity of green, porraceous bile.

In the morning of the 24th we found him lying on his right side: the eye dim; the face pale and cadaverous; the slightest pressure upon the abdomen, the least movement, occasioned the most violent pain. Continual nausea tormented him, which was followed, from time to time, with the expulsion of some gulps of bile. Respiration quick and laborious; pulse very frequent, small; skin without heat. The existence of peritonitis did not admit of doubt. M. Lerminier presumed that the

cause might be referred to an intestinal perforation. (*Forty leeches to the abdomen; oily fomentations; mild sinapisms to the legs in the evening. Linseed tea.*)

The vomiting continued to take place during the day.

25th. Eight o'clock, A. M., abdomen less sensible, but swollen and renitent; no fluctuation could be felt; the limbs were cold; the pulse thready; the eye, however, had still a tolerably natural expression: the intellect was clear; the speech free. (*Blisters to the thighs.*)

He died at five o'clock in the evening.

Sectio Cadaveris, performed fifteen hours after death.—Albuminous flakes, forming false membranes, united together the folds of the small intestine. A turbid, lactescent, very fœtid, serous fluid was effused into the two flanks and into the hollow of the pelvis. The peritoneum underneath the albuminous flakes was strongly injected. The mucous membrane of the stomach was every where white and sound: an equal degree of whiteness was observable in the whole extent of the small intestine; but about a foot above the ileo-cæcal valve there existed five or six oval pimples, as white as the mucous coat surrounding them. The centre of one of these was ulcerated: the base of this ulceration, formed by the serous membrane, had in its centre a round perforation of a line and a half or two lines in diameter. Round these pimples the mucous membrane was covered with several small white, miliary pustules, developed in its interior, and scarcely projecting above its surface.

The great intestine was perfectly sound, as well as the other viscera.

A tubercular mass, of the size of a small nut, existed at the top of the right lung. The exacerbation of the 23d correctly marked the period at which the ulceration took place: the perforation was produced a few hours afterwards.

(*To be continued.*)

On the Morbid Influence of the Spinal Nerves; in a Letter from R. P. Player, Esq., *to the Editor.*

(From the Journal of Science, Literature and the Arts for January, 1823.)

Sir,—At the commencement of the present year you favoured me with publishing in the *Journal of Science*, an account of a morbid connexion which exists between the origins of the spinal nerves, and diseases of parts to which they are distributed. I now beg leave to submit to your notice some results of further attention to this subject.

1. In almost every disease of the upper and lower extremities, of the neck, and of the trunk and its organs and viscera, preternatural tenderness may commonly be discovered on pressure between the vertebræ from which the nerves emerge which proceed to the affected parts, or those spinal branches which are more immediately connected therewith.

2. In diseases in which the circulation is much accelerated, in cases of disease affecting important organs, and more particularly when occurring in old age, this symptom may frequently be discovered to extend along a considerable portion of the vertebral column.

3. Diseases of the head, and its organs, and of those to which the par vagum is distributed, appear primarily to be connected with, if not consequential on, this morbid state, of one or more parts of the spine. The effects of remedies directed to the spine, seem to prove this. When organization is impaired, effects then become causes.

4. In many diseases besides the existence of preternatural tenderness about the origins of the nerves which proceed to the affected parts, this symptom may also be discovered about the origins of one or more of the intercostal nerves on the left side of the spine beneath the scapula, and opposite the upper portion of the stomach.

5. In diseases of females this symptom may, in like manner, be frequently found about the origins of some of the sacral nerves.

6. By the employment of remedies at these parts, some of our most obstinate diseases may be rendered comparatively tractable. For instance, *pain*, in general, may be almost immediately relieved; and the symptoms of gout, rheumatism, phthisis, and cancer, more effectually controlled than by any other means I am acquainted with.

7. The principal remedy is the abstraction of blood in a quantity proportionate to the vascular fulness of the patient. General bleeding being premised, when requisite.

8. Cupping is generally the best mode of removing vascular fulness from the origins of the spinal nerves; this must be repeated according to the recurrence of symptoms or the chronic nature of the case. The glasses should be much larger than they are usually made. Blisters, and similar remedies, become proper, after the due depletion of blood.

9. A recent fit of the gout may be cured by a single abstraction of blood proportionate to the plethoric state of the patient; but in this disease the origins of the intercostal nerves opposite the stomach will commonly require to be relieved, as well as the origins of those which proceed immediately to the affected part or parts.

10. With this precaution, not only the phænomena of gout, but their cause also, appears to be removed; and if organization has not been impaired, the constitution to be completely relieved.

11. In cases of gouty affection of the stomach itself, the abstraction of blood from, or a blister applied over, the origins of the affected intercostal nerves, as the case may require, gives speedy and complete relief.

12. The preceding diseases are only adduced as examples of the advantage of directing remedies *to the spinal brain, and to the origins of the nerves which proceed to affected parts*, and to excite attention to the extensively beneficial application of which this practice appears capable in the relief and cure of *diseases in general;* but it is by no means intended to recommend it to the exclusion of other remedies.

13. Works on Pathology and Physiology, furnish numerous cases and experiments which tend to prove that *pain* and *disorganization*, as well as *impaired function* and *paralysis*, are consequent on causes which interrupt the due transmission of nervous influence to the affected parts. It has been very justly remarked that nature is sparing of causes, but profuse of effects.

I am, Sir,

Your very obedient Servant,

R. P. PLAYER.

Malmsbury, Dec. 4th, 1822.

Remarks on Abortion. By H. W. WARD, Esq. Surgeon.

(From the London Medical and Physical Journal for March, 1823.)

If inquiries become interesting as the subject on which they are made are intricate, the following will, I think, be allowed to be one of this number; and I trust it may be the means of causing the further investigation of a subject of such importance. The cause of the death of the fœtus in utero, is a subject which has for many years occupied the talent of many able men, and it has been referred to a great variety of sources; but the practice which a removal of these causes would in theory seem to warrant has, more frequently than otherwise, proved ineffectual: but, in those cases where the cause of the accident is apparent, it may sometimes be prevented; and, from an attentive consideration of this point, it is that I am enabled to report the success of the mode of practice I have now to lay before the profession. And here, in justice to Dr. D. Stewart, an able obstetric practitioner and lecturer, I acknowledge that I was in a great measure led to this plan of proceeding, by learning from him the efficacy of opiate suppositories in pre-

venting the death of the fœtus in utero, by allaying the irritation of the bowels, which he considered the cause of it.

It was an idea entertained by that able practitioner, the late Sir Richard Croft, that diarrhœa generally preceded or accompanied abortions; but, with deference to that gentleman's opinion, I think diarrhœa is frequently the immediate cause of it: and why I entertain such an opinion, I shall hereafter point out.—Some time ago I was called in to attend a patient, who was in the fifth month of pregnancy, but had no pains that seemed to warrant any idea of labour. I was informed by her attendants, she had never borne a living child, but had aborted three successive times, at this period of gestation; and, from the similarity of symptoms with which she was now attacked, they greatly feared a recurrence of the accident. I regretted that I had not been called in earlier, as a diarrhœa, which was always the most prominent symptom, had existed for four days. As my patient was extremely exhausted by the complaint, I ordered an aromatic cretaceous mixture, with opium; but, in less than eight hours, the fœtus was expelled, dead. The placenta was retained, as is common in such cases, longer than is usually the case in natural labours, and, after waiting an hour, I extracted it. No untoward symptom arose, and my patient recovered.

I was again called on to visit this patient, then also in the fifth month of pregnancy, and labouring under precisely the same train of symptoms as before detailed. As I considered it would be, perhaps, a more fortunate practice, I ordered a laxative draught with oleum ricini; which, however, produced no alteration in the nature or quantity of the alvine evacuations, and I again ordered the cretaceous mixture as before. Notwithstanding this, my patient still continued to labour under these symptoms. I then ordered the cretaceous mixture to be laid aside, and recommended the use of opiate suppositories, which I had hitherto known to be of great service in preventing similar accidents, arising from irritation in the bowels, which I was of opinion was the cause of all the mischief in this case. This plan, however, was attended with no good effect; and, fearing danger existed in delay, I also laid this aside, and wholly depended on large and repeated doses of opium, to be given with a view of allaying the irritation, and preventing a recurrence of the accident. This had the desired effect of subduing the complaint; my patient went through the usual period of utero-gestation, and was delivered of a living child. In the former illness to which I was called, I have no doubt that uterine contraction had begun, and therefore the opium was of no avail; but it must be evident that, if opium be freely given before such action be set up, it probably may—and I think I am right in

saying, generally will,—be successful in preventing it. From what I have said in the preceding part of this paper, it will appear that, in the former illness, the diarrhœa did not abate, and abortion was the consequence; but in the latter, where it was checked by means of opium, my patient went through the usual period of utero-gestation, and was delivered of a living child; a circumstance which never before took place when the diarrhœa was unabated; and hence I think it reasonable to infer this was the cause of the former abortions.

I have said, where the cause of the accident is apparent, it may sometimes be prevented (for we know, if arising from a syphilitic taint, debility, or diseases of the uterus, it may be remedied,) by a removal of these causes: but, on the other hand, cases will occur where all our exertions will be foiled. Among them may be classed those we sometimes find, where the uterus will not allow a sufficient distention to go through the full period of utero-gestation: others, where the uterine vessels do not supply a sufficient quantity of blood for such purpose. And it will also be readily admitted that, where one abortion has taken place, it is generally the precursor to another.

The principal motive I have had in view, has been to stimulate those members of the profession, whose obstetric practice is extensive, to a further inquiry into this important subject.

Maidenhead; November, 1822.

On a new Method of removing Ascarides from the Human Body. By William Howison, M.D., Lecturer on Materia Medica and Pharmacy, &c. &c. &c.

(From the Edinburgh Medical and Surgical Journal for April, 1823.)

The principal cause which gives rise to ascarides, is stated by medical writers to be, unwholesome food with bad digestion. The former, however, can scarcely be said to be the cause, as ascarides are not more prevalent amongst the lower orders of society, than amongst the higher; which undoubtedly would be found to be the case, if the above remark was well founded. On the contrary, we daily meet with abundance of examples of ascarides in the higher walks of life, and amongst individuals who can command, and actually do subsist upon, the most nutritious diet.

Ascarides appear most frequently in individuals of a relaxed habit, and whose bowels contain a preternatural quantity of mucus, or slimy matter. Hence it is stated by medical writers to be a disease most common to children; but that they sometimes prevail in adults to a high degree, particularly in those

who live chiefly on a vegetable diet. I know many individuals above adult age, who have been infested with ascarides for the greater part of their life; but I cannot say that it is peculiar to those living on vegetable food, having met with repeated instances of it in those subsisting upon animal diet, to which they were restricted by dyspepsia; and this disposition to ascarides evidently runs in families. I have known all the individuals of a family infected with them; and therefore am of opinion that they may be said to be hereditary, in the same manner as phthisis pulmonalis, mania, and scrofula.

It is not my intention in these pages, to consider the different means recommended for removing ascarides from the animal economy. We are all aware that, however actively employed, they are tedious, slow, and even uncertain in their effects; that some of them are violent; and that, when long continued, they tend to injure the organs of digestion. Of all of them, however, I may be allowed to say, that I have, in the course of my limited experience, seen most benefit from the administration of the powder of tin combined with the seeds of santonicum, followed up, at regular intervals of a few days, by doses of jalap proportioned to the age of the individual. And to this method I should always give the preference, during the period of infancy and childhood. The removal of ascarides, by means of irritating injections, and it is only such that can prove effectual, or by external applications, as tobacco, turpentine, &c. to the stomach and intestinal canal, appears to me to be inconvenient and indelicate; and the feelings of the people of this part of the world are such, that they will never submit to the use of injections, except in cases of extreme urgency. The practice of the present day confirms this remark. Dr. Heberden says, a repetition of gentle purges alleviates whatever uneasiness ascarides may occasion; but no internal medicines, nor clysters, can certainly be depended upon for extirpating them. Tobacco clysters and others, made of solutions of sublimate mercury, have had little or no effect.

In suggesting the present method of removing ascarides from the human body, it is requisite to bear in mind the following circumstances. 1*st*, That these animals infest every period of life, from infancy to a later period than adult age; 2*dly*, That they are confined to the lower extremity of the intestinal canal, occupying principally, or entirely, that part of the gut extending from the point of the finger, when introduced completely within it, down to the sphincter ani; 3*dly*, That when removed every twenty-four hours, their future generation will be prevented by the obstruction of their means of propagation; 4*thly*, and *lastly*, That this method of removing ascarides is inapplicable to the age of infancy.

The method of removing ascarides already alluded to, must be performed by the patient or individual infested by these insects, in the following manner. Previous to getting into bed, and when completely undressed for that purpose, he is to bring his body into the semibent position, by leaning considerably forwards, supporting himself by means of his left hand resting upon his thigh, or a low table. Having previously besmeared the anus with melted candle-grease, which on such occasions is always at hand, or, what is preferable, with hog's-lard, he is to cover completely the middle finger of his right hand with the same unctuous substance, particular care being taken to have the nail of that finger cut short, so as not to scratch or irritate the internal surface of the rectum. With his body in the position above described, he is next to introduce this finger gradually and cautiously up the rectum, until the hand coming in contact with the sphincter ani, prevents its further introduction.

When the sphincter ani muscle is in a state of relaxation, which it generally is, this step of the operation will be effected with great ease, and without giving the slightest pain. When in a state of spasmodic contraction, which however seldom happens, it then becomes more difficult; and if this contraction should be present to a considerable extent, it will be proper to defer the introduction of the finger until it has completely gone off. The point of the finger within the rectum is then to be rubbed along the whole inner surface of the gut, bringing it gradually downwards, until it comes out at the anus, when the whole mass of ascarides occupying the rectum will be brought out upon the finger near to its point, rubbed into the form of a ball enveloped in mucus and unctuous matter. The first bringing out of the finger, if properly performed, will be found to have removed the ascarides completely, as, upon repeating the operation a second or a third time, nothing will be brought down but mucus, thrown out in great quantity by the increased action of the glands of the gut, irritated by the second or third introduction of the finger.

This operation is to be repeated every night, when, in a short time, the gut will be completely cleared of ascarides. Their further means of propagation being removed, the symptoms produced by them will cease, and the individual be restored to perfect health. I may also remark, that when the ascarides have been several times removed in the above-mentioned way, they are observed to diminish in size; a proof that the older or full-formed ones have disappeared, and that they are succeeded by their offspring, which have not been allowed to come to maturity.

When the rectum is loaded with fecal matter, some obstruction is offered to the introduction of the finger; and the inten-

tion of the operation, the removal of the ascarides, is considerably frustrated.

From what has now been stated it is obvious, that ascarides can be removed from the intestinal canal by a manual, or, what may be termed, a surgical operation, altogether independent of the skill of the physician; and that it presents many advantages over the methods formerly in use. The suffering individual gets rid of the noxious animals, in which his disease consisted, without swallowing unpleasant medicines, which impair the organs of digestion, without wetting his belly by fomentations or liniments, and without distending the intestinal canal, by irritating injections.

9, Nicholson's Square, December, 1822.

On the Products of Acute Inflammation. By THOMAS DOWLER, Esq. Communicated by Sir Astley Cooper, Bart.

(From the Medico-Chirurgical Transactions, vol. xii. part i.)

In John Hunter's excellent work on inflammation, we find some opinions respecting the nature of the substances produced by this process, and the conclusions which that indefatigable author formed, as far as they extended, were, I believe, nearly correct, although the products actually deposited were not made the subject of direct experiment.

Considerable ambiguity has existed respecting the nature of these animal substances, from the various appellations which have, from time to time, been given to them. Thus, that which is commonly known by the name of "coagulable lymph," has been sometimes mistaken for coagulated albumen. The term "coagulable lymph," as applied to what we now understand by fibrin, is evidently a very improper one; for the word lymph, taken singly, has, I believe, been used to express the albuminous part of the blood, and this is very well known to be coagulable; coagulated albumen, therefore, and coagulable lymph might be readily mistaken for each other, although these substances differ considerably in some of their properties. Lymph is also the name of the fluid which is contained in the absorbent or lymphatic vessels, and this is likewise said to be coagulable, although not spontaneously, as is the case with fibrin.

I shall endeavour to prove that it is this latter substance, or fibrin, which is deposited, in conjunction with serum, during the progress of the adhesive inflammation. Chemistry does not yet afford any *certain* means of distinguishing fibrin from albumen, in its solid state; it consequently becomes necessary to have recourse to its physical properties. From one of these

properties the name of fibrin is derived, viz. its always exhibiting a fibrous or filamentous texture; a property which albumen cannot be made to assume, although it be rendered solid by heat, acids, alcohol, or the galvanic fluid, &c. Albumen, again, when coagulated into a mass, is very readily broken down between the fingers into a pulpy form; fibrin, on the contrary, will resist considerable pressure, being firm and elastic. But the principal distinction between these animal products, consists in the fibrin becoming solid spontaneously, when taken from the living vessels, whereas albumen has not this property.

When adhesive inflammation is taking place, the fibrin of the blood, together with serum, seem to be secreted from the vessels: they are both thrown out in the fluid state; but the former has the property of becoming solid soon after it is removed from the influence of those vessels; and in so doing it encloses between its fibres the serum, with which it had been secreted. This change begins to take place very soon after its escape from the circulating system, and does not appear to cease, till a considerable time after it has been removed from the body.

If a blister be applied to the surface of the skin, so as to produce much inflammation, it will occasion a greater or less quantity of adhesive matter to be secreted under it; this will be generally found at the most depending part, clearly proving that it had been thrown out in the fluid form, and had arrived at this situation by its gravitation. Here it gradually assumes the solid state, and during this change it retains between its fibres, (which appear to be flattened and extended) the serum, which is poured out at the same time; and it not unfrequently happens that the fibrin is so abundant, that the whole of the serum is enclosed in it, and that they form together, one solid and opaque mass. If, however, the quantity of fibrin is but small, the solid mass so produced, will be semitransparent, and have a gelatinous appearance; hence it is usually denominated a "gelly blister."

If the solid compound just alluded to be removed from the body, and pressed *strongly* in a fine linen cloth, the serum will be forced out, while the fibrous part remains; this fibrin I have chemically examined and compared with that of the blood, and could not find the slightest difference between them. The serum, so separated, will often contain a small portion of fibrin, which after a time will contract, and again render it of a gelatinous consistence. A substance of a gelatinous appearance is occasionally found about the external layers of the fibrin, in the aneurismal sac, which is probably formed in the same manner.

The solid substance, thrown out in consequence of the application of a blister, bears a great resemblance to the buffy coat of inflamed blood, the nature of which seems not to have been

well understood. M. Deyeux supposed it to be fibrin, altered in its nature, so as to approach to that of gelatin. Fourcroy says, "I do not consider it as demonstrated, that the buffy coat proceeds from fibrin; it appears to me more natural to consider it as coagulated albumen, which carries with it a portion of fibrin, on account of its concressible power."

Again, Dr. Thomson in his work on inflammation, observes, "that the buffy coat was boiled in water, but no solution of it took place; an event which must have happened, had it consisted of gelatine; it exhibited no properties, different from those of the ordinary lymph of the blood."

From the results of some experiments on the buffy coat which were made with a view to ascertain its composition, I am induced to differ in some measure from the opinions of the authors just quoted. The following was one method which I had recourse to, in which I endeavoured to avoid the use of chemical re-agents, as they have been supposed, in such cases, to afford only equivocal results:—Eight ounces of blood were taken from a man who was labouring under rheumatism, and received into two four-ounce vessels; the one being allowed to rest, in order that a buffy-coat might form; while the blood in the other was kept in motion, to prevent its formation. After the lapse of twenty-four hours, the blood that had been kept agitated was washed, in order to separate the fibrin, which, when dried, weighed 12 grains.* The four ounces contained in the other vessel, being washed, and the fibrin collected and dried by the same means (having, however, the buff previously removed), afforded only six grains; consequently, it might reasonably be supposed that the six grains wanting were existing, in some form or other, in the buffy crust. This was found to be the case; for, when it was submitted to pressure, a large quantity of fluid escaped; the mass became evidently of a fibrous structure; and, when dried, it weighed precisely six grains, making, together with the six which were obtained from the crassamentum of this blood, a number equivalent to that produced by the blood in the other vessel. The fluid thus pressed from the buff became solid by the application of heat, and consisted of common serum.

From this, as well as from other experiments, it appears that the inflammatory crust or coat of blood is not altered fibrin, as M. Deyeux thought it to be; neither is it coagulated albumen, as supposed by Fourcroy; nor does it consist entirely of coagulable lymph, as Dr. Thomson seemed inclined to consider

* The proportion of fibrin varies very considerably in the blood of different individuals, and in the same person at different times.

it; but it is composed of a tissue of fibrin, containing between its fibres a very large proportion of fluid serum.

A red globule of blood is known to be formed of a smaller globule enveloped in a vesicle of colouring matter, which can be readily removed by water. The particle which thus remains is precisely of the same size as those of pus, and possesses properties so analogous, that I believe some physiologists have considered them as identical. If this be really the fact, the phenomena of common acute inflammation, when it attacks a part which is not glandular, may be very readily explained.

The modern improvements of the microscope, together with the invention of the micrometer, have afforded us an opportunity of measuring minute objects with accuracy; and Sir Everard Home has found that the particles of albumen, fibrin, pus, and the red globules, differ considerably with regard to their relative magnitudes.* Now, as these substances, when mixed with certain salts and a considerable proportion of water, form the blood, and as they are secreted in the order enumerated when inflammation is taking place, it may be conjectured that the deposition of each chiefly depends upon the diameter of the blood-vessel from which it is poured out. When inflammation takes place, accompanied with effusion, the substance that first escapes is serum; if the inflammatory action gradually increase, and, at the same time, the diameters of the vessels be enlarged, this is followed by fibrin, the nucleus of the red globule, or pus, and, lastly, by blood in an unaltered state, as is seen in the formation of a common abscess. We also find, that when this action is commencing, or exists only in a slight degree, serum alone is effused, as is well seen in hydrocele; but when this fluid is drawn off, and an irritating one injected, the inflammation increases, the vessels become still more enlarged, and serum, together with fibrin, is effused. When a blister is applied to the surface of the body, it irritates and causes inflammation; which, if slight, is accompanied only by a discharge of serum; if it be severe, adhesive matter will be thrown out; and if the inflammation be still more violent, a puriform effusion will take place.

These observations appear to be confirmed, in some measure, by the following circumstances. When inflammation attacks a part, the vessels of which are naturally of large size, it soon goes on to the formation of matter, if not checked by proper remedies; but, on the other hand, if the vessels be very small in the healthy state, they seldom have the diameters so much increased as to allow of the passage of the particles of pus; consequently, when inflammation takes place in such vessels,

* Philosophical Transactions for 1818.

it usually terminates in the effusion of serum, or of adhesive matter. When serous membranes become inflamed, the common products are secreted, which would seem to indicate that these parts do not possess a glandular structure; the blood-vessels, however, being naturally small, the inflammation seldom extends beyond that which terminates in adhesion.

I have used the term serum for one of the products of inflammation for the sake of simplicity; but the constituents of it vary considerably in their proportions to each other; for when it is produced by greatly increased action, it will contain a greater quantity of albumen than when that action is not so violent. This is exemplified in cases of serous effusions, in which we find that the proportional quantity of albumen differs according to the degree of inflammation present.

NOTE ON THE PRECEDING PAPER.

By John Bostock, M.D. F.R.S, &c. &c. One of the Vice-Presidents of the Society.

"As an appendage to the above valuable paper, I beg to state, that I performed some experiments, in the years 1807 and 1808, on the chemical composition of the buffy coat of inflamed blood. The method which I employed was, to remove the layer from the surface of the coagulum, and, after placing it on an inclined plane, to subject it to a stream of water, by which all the soluble matters were carried off, without altering the texture of the insoluble part. The result was, that a quantity of albumen was removed by the water, while a substance was left which exhibited a fibrous texture, and which I concluded to be principally composed of fibrin. At the same time, its appearance, as well as the effect of chemical re-agents upon it, led me to doubt whether it was precisely similar to the fibrin of the blood; and I was disposed to think that a certain portion of coagulated albumen was united with it, and formed a part of its substance.

"The difficulty which I experienced in satisfying myself respecting this point, prevented me from laying my experiments before the Society, along with the others which I performed about this time; and I have never since had an opportunity of resuming them."

Upper Bedford Place, April 17, 1822.

Selected Reviews.

The Study of Medicine. By JOHN MASON GOOD, M.D., &c. In four volumes, octavo.

Continued from p. 351.

(From the Medico-Chirurgical Review.)

CLASS II.—*Pneumatica; or Diseases of the Respiratory function.*

In pursuance of our plan, we come now to another class of vital functions, hardly less important in itself than the first; but, embracing a very limited number of diseases in our author's system. This class is divided into two orders—the first termed Phonica, or affections of the vocal avenues—the second Pneumatica, or affections of the parenchymatous structure, membranes, and motive powers of the lungs. The anatomico-physiological proem to this class, occupies about 25 pages, and does not offer any thing on which we could make any useful remark. We question whether *ventriloquism* should have usurped an eighth of the whole proem—it does occupy three pages and a half out of twenty-five. We must also pass over the whole of the first order, embracing the six following genera:—viz. Coryza, Polypus, Rhonchus, Aphonia, Dysphonia, and Psellismus. These subjects occupy a space of about 50 pages, and afford a considerable stock of curious, and not uninteresting, information.

The second order is also divided into six genera, viz. Bex, Dyspnœa, Asthma, Ephialtes, Sternalgia, and Pleuralgia.

We observe that Dr. Good has considered cough (Bex) as, "at times, as truly idiopathic as any complaint whatever." We confess that we can hardly look upon it as any thing else than a mere symptom of a variety of affections. Indeed, in the greater number of instances, we consider it not a disease, but a remedy, or, at least, an effort of Nature to get rid of some offending matter. In all other cases, we look upon it as the effect of irritation propagated, directly or indirectly, on the air-passages—and, consequently, in every instance, *symptomatic*. Thus, in Dr. Good's first species, BEX HUMIDA, or common cough, "the exhalents (he says) of the bronchiæ are stimulated by an irritation of some kind or other, frequently by a reverse sympathy, &c. whence the bronchial vessels become overloaded and relieve themselves by an expectoration, &c." Surely the cough, or effect of this morbid state of the bronchial vessels should not be nominated the disease itself. The same reasoning will apply to all his other species of cough—even to the BEX CONVULSIVA,

or hooping cough, in which "there can be little doubt that it proceeds, in most instances, from a miasm of a specific nature and peculiar character, having a direct determination to the lungs." Now it will be readily allowed, that the miasm is received into the body long before the cough begins—and, that some change must take place in the mucous membrane of the lungs before the cough is excited—in short, the disease itself must have actually commenced some time before the phenomenon of cough exists, and then, a single symptom is put down for the whole disease! This is one of the imperfections of nosology.

Dr. Good enters little, if at all, into the pathology of the disease, unless the following passage can be considered in that light.

"In infants, it is mostly alarming from its tendency to produce convulsions, suffocation, apoplexy, inflammation of the brain, ruptures, and incurvation of the spine. In adults, it excites pneumonitis more frequently than in children; and in pregnant women has often led to abortion." 595.

He does not once allude to the important investigations of the late Dr. Watt, of Glasgow, whose mind was led to the subject by severe domestic losses in his own family. In Dr. Good's methodus medendi, we do not see any thing new, and we think, he leans too much to the antispasmodic, and too little to the antiphlogistic plan of treatment. Bleeding, he thinks, should only be resorted to in severe cases, "as spasmodic affections are often rather increased than diminished by the use of the lancet." It will generally be found better, he observes, to employ blisters as a substitute. The most effectual remedy, he thinks, is emetics, "whose action tends equally to interrupt the return of the paroxysms, and to keep the lungs unloaded, by producing a determination to the surface." The practice recommended by several writers is, also, shortly stated by our author.

Dr. Good has invented the term LARYNGYSMUS STRIDULUS for what is usually denominated spasmodic croup, and which, he observes, differs essentially from real croup. It is thus defined:—"sense of spasmodic suffocation in the larynx, commencing suddenly, and relaxing, or intermitting; cough troublesome; scanty discharge of viscid mucus." In this disease, he says, we have neither inflammation nor the membrane-like secretion of real croup, while the sense of suffocation is produced, not by obstruction, but by spasm.

"The suddenness with which this complaint commences its attack, forms another mark of distinction between itself and croup, almost as pathognomic as the absence of inflammation, and the peculiar secre-

tion in the latter. There are instances, indeed, in which genuine croup has also commenced abruptly, but these are very rare; for it has usually the precursive symptoms of a slight cough and hoarseness for a day, and sometimes two days, as though the patient were labouring under a catarrh. In croup also, the inflammation, when it has once taken effect, becomes a permanent cause of excitement, and the anxiety and struggle for breath continue with little if any abatement till the inflammation is subdued. In the disease before us, the spasm suddenly subsides in a short time, though it may perhaps return in an hour, or half an hour, or even a few minutes; and in the interval the patient enjoys perfect ease, though the voice is rendered hoarse from the previous straining. Croup is, moreover, an exclusive disease of children; stridulous spasms of the larynx is sometimes found in adults. Those who have been dissatisfied with the name of spasmodic asthma, have, however, treated of it under the name of spasmodic croup, but merely because they have not known how else to distinguish it: for almost every one who has thus noticed it, has acknowledged that it is a different disease, and demands a different plan of cure." 534.

The treatment which Dr. Good recommends is, we believe, judicious. An antimonial emetic should be given as soon as possible, and the diaphoresis which it excites should be maintained for some hours, by keeping the child in bed, and the use of diluents. "The bowels should also be excited by a purgative of calomel." If the emetic does not prove sufficient, or the constrictions should be renewed, "laudanum should be exhibited according to the age of the patient, and a blister be applied to the throat." The great danger consists in mistaking this disease for real or inflammatory croup, in which case, although the emetic and purgative, in the first instance, would do no harm, yet the same cannot be said of the laudanum.

The genera iii. and iv. dyspnœa and asthma, have afforded Dr. Good ample scope for his favourite discussions, respecting nosological distinctions, etymological definitions, and classical arrangement—discussions that will appear dry and uninteresting to most, and absolutely useless to many of his readers. We sincerely wish he had occupied many hundreds of his pages with recent pathological facts, rather than with ancient or obsolete doctrines.

While discussing some modern hypotheses, our author too often passes over in silence, those that are making the greatest noise in the world. Thus, in debating "whether the suffocative tightness of the chest (in asthma) be the result of a spasmodic stricture of the bronchial vessels, spreading thence to the muscles of respiration; or, produced by an infarction of these vessels, from a superabundant effusion from their exhalents," Dr. Good has taken no notice of the controversy maintained for some years past on the Continent, respecting the

pathology of asthma—in which one party, with no mean talent, and many stubborn facts, maintain that the disease is almost invariably dependent on disturbed function or altered structure in the heart. We do not, indeed, subscribe to this doctrine in its fullest extent; but still, we are persuaded, that there is no inconsiderable degree of truth in the melancholy doctrine of Rostan and his followers. In this section, Dr. Good has drawn freely on the standard work of our distinguished countryman Dr. Bree, and, in that, has shown his judgment. He differs from Dr. Bree occasionally, but in a fair and liberal manner.

Genus vi. is on a disease unknown to, or, at least, undescribed by, the ancients. It has received various appellations from modern writers, as syncope anginosa, asthma dolorificum, orthopnœa cardiaca, and angina pectoris. It is termed sternalgia* by Dr. Good, signifying "breast-pang," which is significant enough, as a mere appellation; but, as the disease is now pretty universally acknowledged to be an affection of the *heart*, it certainly appears curious enough, to see it arranged with affections of the lungs. Dr. Good may say, that the function of respiration is disturbed in angina pectoris, but surely so is it, also, in many other diseases, for instance, in pneumonia, which is excluded from this class. Indeed, we are compelled to say, that the farther we proceed, the more convinced we become of

* Sternalgia was applied to this disease many years ago, by M. Baumes, a French writer. We think, that "coralgia" would be a still more proper term—the sense of distress being, almost always, referred to the heart itself.

It is not a little amusing, sometimes, to observe the anxiety of our Parisian neighbours to trace all discoveries which have borne the name of English, to themselves. Thus, in the early volumes of the Dict. des Sciences Medicales, the English are ridiculed for their credulity or ignorance in supposing angina pectoris to be an idiopathic disease; but, by the time they had got to the 52d volume, it was no longer possible to shut their eyes against facts—"l'evidence des faits les a enfin obligés à se retracter." What then was to be done about a disease—"dont la description primitive est généralement attribuée aux Anglais?" What, but claim the discovery for themselves! Four years before Heberden published his account, a *compatriote* wrote a letter to the celebrated Larry, describing angina pectoris. Unfortunately, (there is always some piece of ill luck when the French fail) this letter cannot be found in any *Bibliotheque* in Paris, nor has any author given an analysis of its contents. *Sacre!* It is the only thing wanting to complete the history of French medical literature! "Cette dissertation qu'on doit considerer comme une piece essentielle à l'histoire de la medecine Française." Where is the blame to be thrown for this unaccountable ingratitude to, or negligence of, the poor Peter Perdu? Not on his own countrymen, of course;—no, no—on the *English* certainly! It is entirely the fault of the English that this dissertation is not to be found in the bibliotheques of Paris!—"Pièce que nous devons mettre autant de zèle à faire connaitre, *que les medecins Anglais on ont mis à la laisser dans l'oubli.*" There cannot, of course, be a doubt, that Dr. Heberden travelled all through France, and bought up every copy of this Peter Perdu, which he burnt before he published his discovery. What a happy people are our French neighbours, who can thus draw fuel for their own vanity out of the successes of their rivals.

After all their early scepticism, however, they have at last given a very good account of the disease.

the utter hopelessness of any thing like a natural or perfect nosological arrangement of diseases.

Although we are of opinion that the train of phenomena presented in angina pectoris is produced by various and often dissimilar affections of the heart; yet that, in nine cases out of ten, it does depend on *organic* affections of that viscus, we are well convinced. We have seen in our own practice, and through the kindness of professional friends, a good many instances of this disease, and in all (except one case) there was lesions of structure of some kind, in the central organ of the circulation. In the majority of cases there were ossifications or indurations about the valves or vessels of the organ—in some there was an overloaded state of fat—and in others there was a flaccid and easily lacerable condition of the muscular structure of the organ. We have said there was one exception, though we are by no means certain that this was a *real* exception. It was the late Mr. Price, surgeon, of Walthamstow, by whom we were twice or thrice consulted. He had well-marked angina pectoris, and died in one of the paroxysms. We were not present at the dissection, but it was reported that no organic lesion was found.*

On the Continent, indeed, much confusion reigns in respect to angina pectoris. In France they would not acknowledge, till very lately, any such idiopathic disease, and they said that the angina pectoris of the English writers was nothing but spasmodic asthma.† In Italy, Professor Brera of Padua, has written a treatise on this disease, which he designates by the term "*Steno-Cardia*," indicating the oppressed state and impeded functions of the heart produced by the unnatural compression or encroachment of some neighbouring organ. He does not consider the heart itself as the seat of the disease. In most of the cases related by Valerian Brera, there was great enlargement of the liver, causing much encroachment on the region of the heart, and doubtless embarrassing the free action of that organ; but few, if any, of the cases exhibited the phenomena of real angina pectoris, as known and described in this country. We shall instance the first case as a specimen, somewhat abbreviated.

"A man, 62 years of age, of small stature, but strong and active, had been for several years subject to frequent oppressions suddenly attacking him, which he referred to the middle of the sternum, without feeling any pain in respiration. On the 1st of October, 1804, the oppression returned more frequently and severely than usual, and the patient began to complain of feeling, from time to time, a pricking sensation in the left breast, followed by acute pain, which extended to the left arm. He did not lose his recollection during the

* We have since learnt, however, that Mr. Travers found some organic disease.—*Ed.*

† See the article "angine bronchiale," in the Dict. des Sciences Medicales.

attack, which was short, but he became vertiginous, and his sight was confused. When the paroxysm ceased, there remained a sense of numbness in the whole of the left arm; the pulse was more or less irregular, hard and frequent; bleeding was prescribed, but not submitted to.

"On the 22d in the evening, having walked three miles and being fatigued, he was seized with a violent fit of coughing, and expectorated a little bloody mucus.

"On the 28th, he was seized with a violent pain in the head, and fell down senseless; attempts were made to recover him, but he had breathed his last.

"The brain was found in a healthy state; great adhesion had taken place between the right lobe of the lungs and the pleura costalis, as well as between the pericardium and the left lobe. The heart was of its natural shape and texture, except there being some varices along the course of the coronary veins, *and great dilatation of the right auricle and ventricle.* The liver considerably enlarged and very hard, was displaced from its natural situation, and occupied the whole of the epigastrium; its left lobe was raised in such manner, as to have forcibly pushed up the posterior inferior surface of the heart, and retained that viscus in a state of total compression. The other viscera of the abdomen were in a sound state."

The above case does not correspond with what we have seen in this country under the name of angina pectoris, and after all, as there was actual organic disease of the heart present, we do not see how Professor Brera can decide that the seat of the complaint was in the liver, although it is very probable that the hepatic enlargement conduced to the cardiac affection.

We shall here state the particulars of one of those cases which fell under our own notice, and which we examined after death in the presence of Drs. Lara, Denmark, and some other medical gentlemen.

Case. 19*th July.* The reporter was suddenly called to Mr. Myrtle, aged 40, who was found gasping for breath, while his wife was chafing his chest, and another woman his left arm, which appeared pale and lifeless. His face was pallid, but expressive of great distress —lips livid—forehead bathed in cold sweats—pulse rather quick and weak, but not irregular—inability to articulate from pain in the region of the heart and in the left arm, and also from a sense of suffocation. Without waiting for a history of the case a vein was opened, and by the time that 24 ounces of blood were abstracted, the paroxysm ceased. The patient now gave the following history of his case. In April of the year before, while walking in the street, he was suddenly seized with a violent pain in the guard of the left arm, which presently shot into the breast, accompanied by a loss of breath and a sense of dying. He with difficulty got into a house, and swallowed a glass of spirits, soon after which the fit went off. From that time he never passed a day without one or more of these paroxysms more or less severe. They generally come on about five o'clock in the af-

ternoon, while he is walking home from his employment; but they are readily excited at any time by corporeal exertion or mental agitation. For some time past the pain in the region of the heart is accompanied by a distressing palpitation, and a sensation as if the organ was rolling over and over. He says that he sometimes finds the heart cease to beat altogether. The fit generally lasts but ten or fifteen minutes, sometimes an hour or more. There is always, even between the paroxysms, some degree of dyspnœa. The last was the severest he ever had. For some time past he felt, during the fit, a painful kind of globus ascending to the throat; and at all times he is much troubled with flatulence. He is generally costive, and always worse when purged—a paroxysm is not unusual after a lax motion. For two days after the bleeding he had no attack—an unusual immunity; but they then returned as before, and on the night of the 27th of the same month, he was awakened from his sleep by a severe paroxysm, which terminated his existence, about fifteen months from the first accession of the disease.

The body was opened in the presence of Drs. Lara and Denmark. The lungs were healthy—the pericardium contained about an ounce of serum—the heart was large, fat, curiously mottled on its surface, and remarkably soft and flabby. The parietes of the right auricle were peculiarly thin—the auriculo-ventricular opening on that side rather large—no imperfection in the tricuspid valve—right ventricle passively enlarged, its parietes thin and weak—pulmonary artery and valves natural. The left ventricle, which was full of black fluid blood, presented nothing unnatural except the paleness, softness, and weakness of its muscular structure. Incipient points of ossification were observable on the mitral valve. The origin and arch of the aorta were enlarged, and the coats thickened, indurated, and exhibiting scales of ossific deposition, and still broader ones of substance resembling cartilage. The valves were not diseased. The right coronary artery was distinctly seen opening into the aorta, and on being slit open was found natural. The origin of the left coronary artery was sought for, but in vain. A transverse section of the heart was then made, when the left coronary artery was easily recognised, and a probe was passed along it towards the aorta, where its origin was found completely impervious. The whole calibre of the vessel was much smaller than the other, but not otherwise diseased. No disease in any of the abdominal viscera.

Whether, in the above case, the obliteration of the origin of the left coronary artery can be looked upon as leading to the phenomena of the disease or to the softened condition of the muscular structure of the heart, must be a matter of conjecture. That side of the heart could only have been nourished by inosculation with the other coronary artery. It is probable, however, that the obliteration of the origin of the vessel was of recent occurrence, otherwise the shrinking of the rest of the artery would have been still greater than was found to be the case.

Our friend Dr. Palmer, of Tamworth, has stated, about eight years ago, a case which, though it does not appear to have been one of well-marked angina pectoris, is yet very interesting. The following is only an outline.

"Mr. H——, of Staffordshire, nearly seventy years of age, a very stout and athletic man, had been complaining for six or seven months of uneasiness in the left region of the thorax, extending to the loins, right shoulder, and down the arm as far as the elbow. He could not ascend stairs or steep ground without being suddenly arrested with loss of breath and a severe sense of stricture across the lower part of the sternum. He was much troubled with flatulency and cardialgia. In all other respects he seemed the emblem of health and strength, although "his whole life was passed in an alternate state of excitement from immoderate drinking, and of languor and drowsiness from the exhaustion consequent on it." He swallowed, on an average, two bottles of gin daily! His pulse was, in general, weak and frequent; but hurried and irregular on any mental or corporeal excitement. His father and brother had both died suddenly from unknown causes. On the 26th October, 1814, he was well as usual, took dinner with his accustomed appetite, and walked out into an adjoining field, where he assisted to drag a horse out of a well. He complained immediately afterwards of much exhaustion —leaned against a tree—and sunk lifeless on the ground.

"Dr. Palmer and Mr. Arrowsmith examined the abdomen and thorax only. The lungs were sound—loose part of the pericardium loaded with fat, but the rest of it healthy—three or four ounces of fluid in the pericardium—heart pale, flabby, softened in structure, and completely empty of blood—pulmonary auricle and ventricle enlarged passively—valves of the pulmonary artery rigid—aortic auricle much dilated—mitral valve thickly strewn along its base with points of ossific deposition—the valve itself had lost its natural structure, and resembled cartilage—aortic valves converted into bone —both coronary arteries were become cylinders of bone—the root of the aorta one-third larger than natural—no ossification of the cartilages of the ribs. The abdominal viscera were sound, and permission could not be obtained to open the head."

We agree with Dr. Palmer that the above case "in its leading characters and termination more strongly resembled angina pectoris than any other organic disease of the heart with which we are acquainted—not, however, that affection in its simple and more rare form, but complicated with valvular disease of the aortic portion of the heart."*

* See the cases of angina pectoris published by Dr. Black of Newry, in the Memoirs of the Medical Society, vol. iv. and also in the seventh volume of the Medico-Chirurgical Transactions, for instances analogous to the above. The experience of Dr. Jenner and Allan Burns also is in favour of the organic cause of the disease. Jurine and Desportes, who have written two excellent dissertations on angina pectoris, consider the disease as attributable to functional disorder of the heart and lungs, and as rangeable among the *neuroses*.

We have made little or no allusion to several writers who regard this disease as totally independent of any organic affection, but rather as occasioned by spasm or the irritation of some translated disease, as gout, &c.*

Among these are Darwin, Berger, Butler, Heberden, Bergius, and Dr. Parr. But dissections are now more frequently and more carefully made than in former periods—and organic diseases are more readily detected. Dr. Good appears to view angina pectoris as "not originating necessarily in any structural derangement of the organs affected," and his nosological definition seems to place it in connexion with the respiratory rather than the circulating organs. In this we cannot agree with our esteemed author. We shall quote his methodus medendi.

"The mode of treatment which I have found most successful consists in putting the patient immediately in an inclined, rather than a recumbent position, with his head raised high. He should instantly take an emetic of whatever may be given most expeditiously, though the antimonial preparations form the best medicine for this purpose, as producing a longer action. As soon as the patient rejects, he may be allowed a little warm water, though this should be administered to him sparingly. The diaphoresis hereby usually induced should be assisted by a moderate warmth of bed-clothes, and particularly by placing the patient between the blankets; and if the constrictive pain or difficulty of respiration still outlasts the sickness, opium intermixed with ether, camphor, or other diffusible antispasmodics, should be employed pretty freely." 589.

Our own view of the nature and cause of the disease precludes, of course, all expectation of cure. We have seen the disease retarded in its progress, but no case prevented ultimately from terminating fatally. There are, we acknowledge, some cases on record, that seem to prove the curability of the disease. Thus, Dr. Parry himself, who placed its cause in ossification of the coronary arteries (which surely he did not believe to be removeable) mentions the case of an elderly patient who had laboured under angina pectoris, but who had wholly or nearly recovered from the disease by the use of the Bath waters. Credat Judæus! Dr. Robert Cappe also relates a case in the fourth volume of the Medical and Physical Journal, which he asserts was angina pectoris, and cured by nitrate of silver. But we are confident the disease was not angina pectoris. Dr. Cappe never saw the patient in a paroxysm, though the fits always returned once, and sometimes very often daily. We think, therefore, that Dr. Cappe had very little curiosity or anxiety about the disease at all. In fact, we do not recognise a single symptom of angina pectoris in the case, ex-

* See New Med. and Phys. Journal for December 1814.

cept that of "gasping for breath," which every body knows attends many other diseases, and winds up almost all. The reasonings which led Dr. Cappe to employ lunar caustic in the cure, we do not well comprehend; but the fact on which they were based was this—that in the year 1793, when galvanising the nerves and muscles of frogs, he found the *latter* restored to energy by touching them with argentri nitras, after they had ceased to obey the electric excitation.

A well-marked case of angina pectoris is related by Dr. Palmer, in the eighth volume of the New Medical and Physical Journal, for December 1814, where considerable alleviation of the disease was obtained by instituting a permanent drain from the region of the heart; by abstinence; quietude of mind and body; moderate exercise in the open air; antacids; bitters; aromatics; and gentle laxatives. These measures attended to during the intervals of the paroxysms, may do much to procrastinate the fatal catastrophe in some cases, though they will prove utterly inefficacious in others.

It may be asked what ought we to do in the paroxysm? Considering the weakened or disorganized state of the heart, we would *not* recommend bleeding, though it will be observed that we bled the patient whose case is narrated. But he appeared to be dying, and we bled him without having time to reflect on the nature of the disease. As it happened, he was relieved by it at the time; but we repeat it that we have since seen bad effects from general bleeding. If called to a patient labouring under the immediate attack, we should be more inclined to exhibit gentle cordials, with cool and fresh air, than any other class of remedial agents. At the same time there may be cases where the heart becomes, as it were, overpowered with blood, as in exercise, &c. and in such cases it may be proper to draw off a few ounces just to relieve the local plethora about the heart for the time. It is curious that great flatulence in the stomach is an almost invariable attendant on angina pectoris, and not a little aggravates the complaint. Indeed we have reason to believe that it occasionally excites the attack. It is therefore of great consequence to obviate this symptom by every mean in our power. Dr. Percival informs us that nothing relieved the paroxysms so instantaneously as venesection and *vomiting;* but death may take place before emetics may be made to act. Dr. Parry, as may naturally be expected, was a strong advocate for sanguineous depletion during the paroxysm, but he desires it to be done under the following precautions:—"blood should be taken from a small orifice, the patient being placed in the horizontal position, while the physician is to keep his finger on the pulse to decide on the limits to

which venesection is carried." Rubefacients, frictions, and pediluvia may be serviceable.

Class III. *Hæmatica; or Diseases of the Sanguineous Function.* This class contains four orders; viz. fevers, inflammations, eruptive fevers, and cachexies. The physiological proem prefixed to this class descants, 1st, on the machinery of the sanguineous system; 2dly, on the moving powers; and 3dly, on the nature of the fluid circulated. It consists of 33 pages, and exhibits much research, little originality, and some error. We deem the following passage, just under our eye, to be an example of the last ingredient.

"It may be still further observed, that, in a state of inflammation, the pulse of the inflamed part, in consequence of local excitement, is much more frequent than that of the heart or of any other organ. *Thus in a whitlow, the radial artery may give to the finger a hundred pulsations in a minute, while not more than seventy strokes may be exhibited in any other part of the system.* The rapidity of the pulse is in this case usually in proportion to the degree of the inflammatory action; and hence, if the system should labour at the same time under ten different inflammations in different parts or organs of a different structure, as glands, muscles, and membranes, it is possible that it may have so many different seats of pulsation taking place at such different parts at one and the same time, while all of them are at variance with the pulsation of the heart." 17.

We have probably paid as much attention to the phenomena of the pulse and the circulation in general as our brethren, or as Dr. Good himself; but in the course of nearly thirty years' observation, we never witnessed a single instance of the *number* of pulsations in an inflamed part varying from the number of contractions in the left ventricle of the heart. And if Dr. Good will show us a single example of the kind marked by us in italics, we will pay him down one hundred guineas—if ragged reviewers can be supposed capable of mustering up such an immense sum in these hard times. We have seen a very few instances, in diseased states of the heart, where the pulsations at the wrist were *less* in number, (generally just half the number) than those at the heart—but never a single instance where they were *more* in number. Indeed we consider the latter to be a physiological impossibility, unless in cases where there is one heart in the chest and another at the elbow. That the diameters of vessels and the *degrees* of their throbbing increase considerably as they approach a phlegmonous tumour or acute inflammation, is well known; but the pulsations are *synchronous* with the other tangible arteries and with the ventricular contractions.

Dr. Good appears to adopt the doctrine that the heart acts in the double capacity of a forcing and sucking pump, and that it is

the main, if not the sole moving power of the circulation under ordinary circumstances. He seems also to embrace Dr. Carson's hypothesis respecting the resilient power of the lungs in expanding the chambers of the heart; but as Dr. Carson is very generally allowed to have failed in explaining the circulation of the fœtus in utero, where the said resiliency has not come into play, we think the whole hypothesis must resolve itself shortly into its original constituent principles, like all other things on earth or in the air. It must be confessed that Dr. Good is sufficiently indulgent to new theories and hypotheses—thus Sir Everard Home and others "have established beyond a controversy" the communication between separate organs, "though we can trace no intercourse of vessels." The kindness of the cardiac portion of the stomach in transmitting fluids to the spleen, without the intervention of vessels, is certainly very extraordinary—and still more so the mystic or rather the magic communication between the stomach and bladder. All we can say at present is, that experiments as well as experience are fallacious now as they were in the days of Hippocrates.*

We gladly emerged from the labyrinth of microscopical observations on the blood; but PYRECTICA or FEVERS stared us in the face, and the very sight of the *word* proved the *thing* contagious, without any arguments, for we instantaneously felt the cold chills of its intermittent form creep over our frame! It requires, indeed, no small degree of courage as well as curiosity to wade once more through the pyrectic theories of the schools; —the concoction and critical evacuations of Hippocrates and Galen—the acid, alkaline, effervescent despumations of Van Helmont—the lentor and error loci of Boerhaave—the spasm of Stahl, Hoffman, and Cullen—the sthenia and asthenia of Brown —the sympathetic associability of Darwin—the inflammatory hypothesis of Marcus—the cephalitis of Clutterbuck—the excitement and collapse of Armstrong—the gastro-enteritis of Broussais—and the new old doctrine of Stimulus and Contrastimulus now flourishing in Italy! Yet with all these theories before us, the hue and cry both at home and abroad is—"give us general principles to act upon; and away with your empirical treatment of each case according to its symptoms." If we ask what principle will ye have, gentlemen?—one calls out for Armstrong, another for Broussais, a third for Clutter-

* Della Torre made out the corpuscles of the blood to be flat circles or rings with a perforation in the centre—Hewson represents them as hollow or vesicular, with a dot of red colouring matter in the centre, so that they were doubtless "the wheels of life moving on iron axles;"—but "M. Baur," says our author, "has ascertained, he thinks, that it is not the centre of the globule that is dotted, but its outline that is surrounded with colouring matter; so that instead of being annular wheels with iron axles, they are spherular wheels with iron tiers." 33. Risum teneatis amici?

buck, a fourth for old Cullen, while several sober-looking gentlemen shake their heads, and observe that neither principles nor practice can yet be positively defined, and till then we must "obviate occasional symptoms" as they arise, even at the risk of being designated empirics. In truth, none are greater empirics than those who attempt to act on fixed principles in medicine, where no fixed principles are established. Thus, if a Broussaian comes to a fever patient with evident symptoms of determination to the head, and yet, true to his master's principle, applies leeches to the epigastrium—he is an empiric. If a disciple of Clutterbuck, on the other hand, finds a patient in fever, with pain, tenderness, and fulness about the epigastrium, for which he shaves and blisters the head, *on principle*—he is an empiric. The red hot phlebotomist who bleeds in all cases of fever—because it is fever, is just as much an empiric as his predecessor, the Brunonian, who "never wet a lancet in fever," because all fevers were asthenic. In short, if there be any thing like a well-founded principle in medical science, it is this ridiculed principle of treating every individual case according to the phenomena which it presents at each varying stage, and in each varying constitution, instead of on a pre-established principle which is more likely to be founded in error than truth, and which, if ever so just, could not apply even to the majority of cases.

In the age we live in, two main doctrines respecting fever divide the medical world, one of which is subdivided into certain sects or parties. The first of these doctrines, and that which has still the greater number of adherents, contemplates fever as an idiopathic disease, and though frequently not *necessarily* connected with inflammation of any particular structure in the body. The other doctrine, the disciples of which are increasing, views fever as nothing more than a general or constitutional expression of a local disease—which disease is, of course, inflammation. The converts to this doctrine or reformation have, as is not unusual, split into sects, each asserting that its own dogma is the only infallible one. These sects are three in number. One, the followers of Marcus, regards the local inflammation as sometimes in one organ or structure of the body, sometimes in another—but always present as the basis of fever somewhere.* The second class, with Broussais at

* "On this ground it is that Professor Marcus of Bavaria, who has contended with similar strenuousness for the identity of fever and inflammation, has regarded *all inflamed organs as equal causes;* and is hereby enabled to account for the different kinds of fever that are perpetually springing up before us. Thus inflammation of the brain, according to Marcus, is the proximate cause of typhus; inflammation of the lungs, of hectic fever; that of the peritoneum, of puerperal fever; and that of the mucous membrane of the trachea, of catarrhal fever." *Good*, vol. ii. p. 59.

their head, have fixed the origin or focus of fever in the mucous membrane of the digestive organs, whence it radiates on all parts of the system, and produces the phenomena hitherto falsely attributed to a kind of entity or imaginary being denominated idiopathic fever. The third party of phlogotics (as they may be called in contradistinction to the pyrectics, or idiopathic party) are pretty numerous in this country, and consider Dr. Clutterbuck or Dr. Ploucquet as their founder. It is well known that they confine the *fons et origo* of fever within the parietes of the cranium and vertebral canal, making phlogosis of the brain, its membranes, or its appendages the exclusive proximate cause of fever.

Now it is quite impossible that these various doctrines can be all right. And yet we think they are all right—*at times.* It is the Procrustic bed, on which each party stretches or cramps his favourite theory, that deprives them all of their natural proportions, and renders them all more or less deformed.

Of the various theories of fever, we believe that those which give most latitude to the causes, most variety to the features, and most contingency to the consequences or post-obit phenomena, will come nearest the truth. We consider, therefore, that the *first* doctrine to which we have alluded (though not without faults, some of which we shall presently point out) approximates most to a true estimate of the nature of fever. We do not think that the fundamental principle of this doctrine has ever been more clearly or more succinctly stated than in the following words of Dr. Fordyce:—

"'Fever, therefore,' says he, 'is a disease that affects the whole system; it affects the head, the trunk of the body, and the extremities; it affects the circulation, the absorption, and the nervous system; it affects the skin, the muscular fibres, and the membranes; it affects the body, and affects likewise the mind. *It is, therefore, a disease of the whole system, in every kind of sense.* It does not, however, affect the various parts of the system uniformly and equally; but, on the contrary, sometimes one part is much affected in proportion to the affection of another part.'"*

The validity of the above doctrine has not, we imagine, been shaken by all or any of the investigations which have taken place since the days of Fordyce. The statement quoted will certainly apply to fever when regularly formed; but if we look more narrowly into the chain of phenomena which connects this state with the first application of the cause of the fever, we shall probably be inclined to relax a little in favour of those doctrines which assign a "*local* habitation and a name" to the disease. If a person, for instance, be carefully watched after

* On Fever, Dissert. i. p. 28.

exposure to the febrific miasm issuing from marsh or human sources, he will unquestionably exhibit phenomena that indicate a local or circumscribed sphere of morbid action, in the earliest movements of the disease. In whatever way the poison gets into the system, the sensorium commune, as might naturally be expected, appears first to feel its effects. Our own observations and our own feelings would induce us to think that the *mental* faculties or functions are impaired at an earlier stage than any of the grosser functions—implying that the brain and nervous system are primarily implicated. It is very easy to see that any functional ailment in this class of organs (whose influence on all parts of the body is so extensive) must so very promptly derange the various other functions of the human frame as to render priority of lesion quite undistinguishable by the great mass of observers—and thus give rise to the idea of general and simultaneous disorder throughout the whole system. Thus far then we agree with Ploucquet and Clutterbuck in making the brain and its appendages the primary seat of fever. But so far from considering this primary affection as inflammation, we regard it as quite the reverse—at least the phenomena presented by it (the only proofs we can have in that stage) are diametrically opposite.

When, indeed, that operation takes place in the economy which has aptly enough been termed *reaction*, a state obtains in the brain, the viscera, and the vascular system, very closely allied to, and very often terminating in, unequivocal phlogosis. Whether we designate this condition (which precedes, resembles, and, we apprehend, induces inflammation) by the terms excitement or irritation, is not perhaps very material. It is known in the living body by a lower degree of those functional disturbances which attend idiopathic inflammation—and in the dead body by a turgescent state of the capillaries of the part. Both this state and the actual inflammation into which it often runs, are also known to the observant practitioner by a therapeutical phenomenon—namely, that, whereas you may *control* them by the usual antiphlogistic means, you will very seldom be able to *remove* them with the same facility and in the same space of time, that you would idiopathic inflammation—and why? because they are produced by an effort of the constitution to resist or repel a deleterious agent—which effort requires, in general, a cycle or period varying according to the nature of the morbific agent and the powers of the constitution. The poisons of plague, of typhus, of variola, and other diseases offer ample proofs and illustrations of this position.

That local inflammations do frequently occur in fevers of every description, and in almost all protracted and fatal cases, is now proved beyond the possibility of doubt, by the great dis-

turbance of function in the organ or organs before death, and the manifest alterations of structure on dissection. Death, however, does occasionally take place without leaving any trace of inflammation—and that in three different ways, viz. 1st, where the force of the febrific cause or miasm is so great, or the constitution so weak, that no regular reaction of the system ensues, and life is destroyed by a gorged state of the vessels of the brain and viscera. 2dly. Where the reaction is so violent that the great vital functions are exhausted or overwhelmed before any of the usual appearances of inflammation have had time to form. 3dly. Where the disease is long protracted, from whatever cause, and the patient gradually sinks by a slow exhaustion that leaves little or no trace of structural alteration after death.

If then these local inflammations be contingencies or consequences rather than causes or essential concomitants of the fever, it may be asked why do they take place in one organ or class of organs rather than in others—or why do they take place at all? It may be answered, that when the cause of fever (say the poison of typhus, variola, or plague) is applied to the constitution in sufficient quantity, that process termed reaction of the whole, but especially of the vascular system, must take place, consistent with the laws and safety of the animal economy. But this reaction or great vascular commotion is not without its attendant danger. Even in the best constitutions, where every organ seems endued with its just proportion of power, and the equilibrium of the functions is perfect, the whole system experiences a great shock during fever, and the patient is left in a state of vast debility and exhaustion. What then is naturally to be expected when one organ or class of organs is previously weak or imperfect in its functions? We see the consequence too often, when the vascular excitement of measles, variola, or scarlatina—nay, the common symptomatic fever from a wound or accident, excites phthisis in persons whose lungs are previously unsound. Upon this principle only can we account for the balance of irritation or inflammation inclining sometimes to one organ, sometimes to another in fever—and we see it almost invariably fall upon that structure in greatest force, which was previously predisposed to disorder from the habits or original constitution of the patient. The alimentary canal and the brain are unquestionably the organs most frequently affected in fever, as we might naturally expect, from the intemperate habits and turbulent passions of society. The followers of Ploucquet and Clutterbuck will more easily persuade themselves than others that the brain is the primary organ affected, and the other lesions merely secondary. On the other hand, the Broussaians may argue for the priority in the abdominal viscera; while the unbiassed observer will probably join us in the view which we

have taken of the contingency of inflammation in fever. Indeed, we think that a careful consideration of the various remote *causes* of fever;—of the various *forms* which it assumes in different epidemics, in different climates, and in different subjects —of the various *modes of treatment* which have succeeded in the disease—and of the various *post-obit appearances* recorded by practitioners, might have deterred any but the most determined theorists from setting up such circumscribed systems of pathology as Drs. Ploucquet, Clutterbuck, and Broussais have done.

The foregoing brief exposé of the nature of fever may not be inappropriately followed by a few words on the general principles of treatment. A considerable range of observation, and a very attentive consideration of the subject have long since led us to compress the voluminous *principles* laid down by authors into a very narrow compass—we had almost said into a nutshell. Viewing fever then as an effort of Nature to resist some morbid impression or expel some morbid miasm, we consider it the grand business of the practitioner to watch the several *functions*, and by relieving them when oppressed, thus protect the *structure* of the parts. In this office he will sometimes be obliged to control the action of excited organs, and excite the action of torpid ones—and if there be a general principle in the treatment of fever, this is the one most universally applicable at the bedside of the patient. This principle scarcely requires elucidation to render it comprehensible by the meanest capacity; but we shall apply it to a few of the leading phenomena of fever.

We come to a patient at a very early period of fever, and where the individual, for instance, has been exposed to the contagion of typhus or concentrated marsh poison. We find the surface cold and pale, the pulse weak and quick, the breathing hurried and laborious, a sense of oppression at the precordia, disturbed intellect, diminished secretions. What can be more evident than that the whole balance of the circulation and of the excitement is here deranged. The skin and secretory organs are torpid—and the blood is oppressing the vital viscera. What does the principle in question point to under such circumstances?—To the very best mode of treatment. Apply the tepid bath to the surface to draw the circulation there—give warm diluting drinks for the same purpose—and act upon the various abdominal secreting organs, and on the skin, in order to restore their functions, which are for the time suspended.

But in a short time we see the whole phenomena reversed, as it were. We find the surface burning, the face flushed, the arteries throbbing, the capillaries injected, the intellect excited to delirium, and still the secretory organs locked up. Here then we are obliged to almost exactly reverse the means we

were before pursuing. The surface must be cooled by fresh air or gelid water—blood must be drawn if the excitement run high, or any one organ appears to suffer more than the others—and the bowels must still be acted on. By these means, and without these means by the powers of Nature, a third state or condition of the system occurs, differing materially from either of the preceding. The skin and the secreting organs act, and the whole phenomena of fever suffer a great diminution or entire, though temporary, solution. In this third state what are the indications?—To do nothing at all. But in the fourth state or remission, what do the *principles* of our systematic sticklers point to? To the use of an empirical remedy, bark or arsenic, which their principles never discovered or dreamt of, and cannot even now explain!

But instead of these stages or conditions of the system being pretty distinctly marked, and sufficiently regular in their succession, we have too often, as in continued fevers, a medley of all the stages at the same moment, one part of the system being in a state of collapse or torpor, another in a state of excitement, and a third in a state of relaxation. Now in these amalgamations we do fearlessly aver that we have no other safe rule to follow but that of counteracting symptoms as they arise, and as they happen to predominate in one organ or set of organs more than in others. Our paramount duty, we repeat it, is to diligently watch the *functions* of the various organs, and, whenever any of them appear to labour disproportionately, to endeavour to relieve them by drawing off the current of the circulation and of the excitement by local or by general means. By a careful conduct of this kind, and by avoiding the MEDECINI PERTURBATRIX, except upon very urgent occasions, we shall give Nature the fairest chance of going safely through the peculiar cycle which the disease, the constitution, or the climate may require for the work of restoration.

The long laboured, and almost endless descriptions of the ever varying forms, features, and shades of fever, (which are never the same in two individuals) are far better calculated to make a book than a practitioner. In the first place they cannot be retained in the memory; and in the second place, they would be useless if they were retained. The simple rules which we have sketched out here, are those which we have found most applicable at the bed-side of sickness, and we apprehend that they will prove of no mean advantage to the young practitioner. We have, of course, only alluded to the indications, leaving the means of fulfilling these indications to the choice or discretion of the medical attendant. They are sufficiently well known.

And now to return to Dr. Good. In his observations on the remote causes of fever, he seems to coincide with Cullen, that

the two great sources are human and marsh effluvium, viz. contagion and a certain aerio-terrestrial influence or agent to which we have given the name of marsh effluvium or malaria. Dr. Good very properly remarks, that there are a very considerable number of other causes than the above, which are capable not only of predisposing to, but of exciting the disease, as cold, intemperance, heat, the passions, and numerous other agents. We cannot agree entirely with the following sentiment of our author.

"And notwithstanding that it becomes us to speak with diffidence upon a subject respecting which we are so much in want of information, I may venture to anticipate that the evidence to be advanced in the ensuing pages upon the general nature and diversities of fever, will show that there is more reason for believing that the febrile principle produced by marsh and human effluvia is a common miasm, only varying in its effects by accidental modifications, and equally productive of contagion, than that it consists of two distinct poisons, giving rise to two distinct fevers, the one essentially contagious, and the other essentially uncontagious, as contended for by Dr. Cullen." P. 64.

Although we are advocates for *contingent* contagion in certain cases of marsh fever; yet, considering the very marked difference which almost invariably obtains between these and fevers from human contagion, we cannot admit the identity of miasm in both classes of disease. We agree with Dr. Good that "the marsh and oozy soil of inhabited countries is necessarily a combination of animal and vegetable matter," a fact to which most of our writers on marsh fevers seem, of late, to have shut their eyes, for they speak of nothing under such circumstances, but decayed or decaying vegetable substances; yet the product of this vegeto-animal poison we look upon, if the expression of its effects is to be taken into account, as different from the poison generated by the living body labouring under disease, as, for instance, under typhus fever.

With the exception of the 4th corollary in the following summary, we agree with our learned author in the justness of his conclusions.

"1. The decomposition of dead organized matter, under the influence of certain agents, produces a miasm that proves a common cause of fever.

"2. The whole of these agents have not yet been explored; but so far as we are acquainted with them, they seem to be the common auxiliaries of putrefaction, as warmth, moisture, air, and rest, or stagnation.

"3. The nature of the fever depends, partly upon the state of the body at the time of attack; but chiefly upon some modification in the powers or qualities of the febrile miasm, by the varying propor-

tions of these agents in relation to each other, in different places and seasons. And hence, the diversities of quotidians, tertians, and quartans; remittent and continued fevers, sometimes mild and sometimes malignant.

"4. The decomposition of the effluvium transmitted from the living human body, produces a miasm *similar to that generated by a decomposition of dead organized matter*, and hence, capable of becoming a cause of fever under the influence of like agents.

"5. The fever thus excited, is varied or modified by many of the same incidents that modify the miasmic principle when issuing from dead organized matter; and hence, a like diversity of type and vehemence.

"6. During the action of the fever thus produced, the effluvium from the living body is loaded with miasm of the same kind, completely elaborated as it passes off, and standing in no need of a decomposition of the effluvium for its formation. Under this form it is commonly known by the name of febrile contagion. In many cases, the secretions are alike contaminated; and hence, febrile miasm of this kind is often absorbed, in dissection, by an accidental wound in the hand, and excites its specific influence on the body of the anatomist.

"7. The miasm of human effluvium is chiefly distinguishable from that of dead organized matter, by being less volatile, and having a power of more directly exhausting or debilitating the sensorial energy, when once received into the system. Whence the fevers generated in jails or other confined and crowded scenes, contaminate the atmosphere to a less distance than those from marshes and other swamps, but act with a greater degree of depression on the living fibre.

"8. The more stagnant the atmosphere, the more accumulated the miasmic corpuscles from whatever source derived; and the more accumulated these corpuscles, the more general the disease.

"9. The miasmic material becomes dissolved or decomposed in a free influx of atmospheric air; and the purer the air the more readily the dissolution takes place; whence, è contrario, the fouler as well as more stagnant the air, the more readily it spreads its infection.

"10. Under particular circumstances, and where the atmosphere is peculiarly loaded with contamination, the miasm that affects man, is capable also of affecting other animals.

"11. By a long and gradual exposure to the influence of febrile miasm, however produced, the human frame becomes torpid to its action, as it does to the action of other irritants; whence the natives of swampy countries, and prisoners confined in jails with typhus contamination around them, are affected far less readily than strangers; and, in numerous instances, are not affected at all.

"12. For the same reason, those who have once suffered from fever of whatever kind hereby produced, are less liable to be influenced a second time; and, in some instances, seem to obtain a complete emancipation." 77.

We cannot enter on a consideration of the individual species

and varieties of fever detailed in an able and interesting manner by our author, in the first 200 pages of the second volume. The student will there find a great deal of information collected from various sources—the lecturer, much to furnish out his discourse to his pupils—and the practical physician something to smile at, occasionally, in the conflicting doctrines and hypotheses brought forward each in its turn, "to strut and fret its hour upon the stage."

The third order of this class embraces the subject of inflammations, and occupies more than 300 pages of letter press. It is not much to be expected that Dr. Good should loose or cut the Gordian knot of inflammation. We are much mistaken if the following summary of "the little that we know upon the subject" might not have been comprised in a still smaller space than our author has allotted to it.

"The little that we know upon the subject may, perhaps, be comprised in a few words:—The standard of firm health is the best guard against inflammations of every kind, or the state in which a man is least susceptive of them; and a deviation in either direction, whether towards a habit of *entony* or of atony, *capacifies* him for breeding them. But it does not *capacify* him equally; for in the latter case they are produced far more easily and generally than in the former. In *fibrous entony*, obstruction appears to take place, and inflammation to follow, from an increased tendency to constriction and rigidity in the muscular tunic of the arteries generally, and an actual constriction in those of the part affected; in consequence of which the diameter of the tube is diminished, and the blood, though urged by a stronger impetus from behind, works onward with less freedom than usual. In fibrous atony, obstruction takes place from the relaxed and yielding state of the vessels which admit grosser corpuscles of the blood than what naturally belong to them, and thus become accessary to the error loci of the Boerhaavian school. But a mere error loci is not sufficient for inflammation: since the erratic corpuscles are readily forced back, or pass diagonally into larger vessels from the numerous anastomoses that prevail in the arterial system. Of this we have a pertinent example in the red suffusion which frequently takes place in the tunica albuginea of the eye, which is often an effect of weakness alone, is unaccompanied with heat or pain, and consequently with inflammation, and perhaps passes off by the next day. In addition, therefore, to the relaxed state of fibres and the error loci before us, there must be something of that irritability which is so frequently an attendant upon relaxed and mobile organs, and which produces spasmodic and contractile action in a far higher degree, though, perhaps, in irregular fluxes and refluxes, than any habital firmness or rigidity of fibre does at any time." 233.

If Dr. Good was able to form any clear idea of the foregoing theory of inflammation, it is far more than we have been able to do. Well might he say in the succeeding paragraph, "con-

cerning the proximate cause of inflammation there is yet much to be unravelled." We have marked in italics a few words which show Dr. Good's propensity towards unusual terms and expressions.

Our author has given us a tolerably fair statement of what we know respecting the remote causes, and a few of the laws which seem to govern inflammation; yet even in this portion of the work we think we could "pick some holes in his skirt," were we inclined to be critical. Thus in our author's zeal to show the "wise and beneficent laws of Providence," as exemplified in the tendency of suppurations to point outwards, we imagine Dr. G. has rather overstepped the mark.

"Thus," says he, "if an inflammation attack the peritoneum covering an intestine, and adhesions are hereby produced between the two, the inflammatory action works upwards through the thick walls of the abdominal muscles, while the proper coats of the intestines in most instances remain sound." 239.

Now in the course of nearly 30 years' experience, we do not recollect to have ever seen an instance where, in peritoneal inflammation, the inflammatory action worked its way outwards; but we have seen many instances where erosions of the intestinal coats obtained, and where there was extravasation of fæcal matters into the common cavity of the abdomen. So far then from Dr. Good's position being the general rule, we should be inclined to look upon it as the exception.

Again, in the next page, our author lays it down, that "simple or *healthy inflammation* is a *remedial process* for restoring a part to soundness, when affected by a morbid impression that has a tendency to injure or destroy it." Yet, in the same page, when speaking of the treatment, he lays it down as a fundamental principle, that we should endeavour "to make a new impression upon the part, and to oppose a healthy or remedial, to an *unhealthy and mischievous, action.*" Here then, *healthy inflammation*, which is a *remedial process* in the beginning of the page, is an *unhealthy and mischievous action* at the close of it. There appears some incongruity in these statements.

Of the individual inflammations it will not be in our power to speak. In general Dr. Good has amassed a great body of information on each particular subject; but here and there we observe a very defective state of his pathological researches. This defect is particularly conspicuous in his chapters on inflammation of the lungs and their investing membranes. Thus, in peripneumony we have the terminations in effusion, suppuration, and gangrene: but he does not notice the consolidation or hepatization, as it has been called, of the pulmonary structure; a very common termination of inflammation, acute and chronic.

The following passage contains the whole of our author's pathology of pleurisy.

"Like the preceding species also, pleurisy terminates in resolution, suppuration, and gangrene. The former is the ordinary and most favourable issue. The last occurs rarely; but suppuration is by no means uncommon; in which case, if the abscess do not point outwardly, an empyema will necessarily follow; and the formation of pus is indicated by a remission of the pain, one or more shivering fits, and, in some instances, a sense of fluctuation. This, however, is a termination far more common to pleurisy from external injuries, than from internal causes." 366.

Yet, in nine cases out of ten of fatal pleurisy, the termination is by sero-purulent effusion into the cavity of the chest, without any thing like an abscess pointing outwardly. It is well ascertained, also, that in all cases of acute pleuritis, there is more or less of serous effusion, which becomes absorbed, in the event of recovery, leaving or not, according to the extent of the disease, adhesions between the pleura pulmonalis and pleura costalis. In a new edition, Dr. Good must extend greatly his pathological researches; and on the subject of pulmonic affections, he cannot draw from a better source than the investigations of Laennec and Baron.

By the way we have some fault to find with Dr. Good in his treatment of pulmonary inflammation. "The best, the easiest, and even the natural cure of peripneumony is expectoration." We cannot subscribe to this doctrine. It is the *natural* cure no doubt, because Nature has no other way of unloading the vessels, without great danger. But by a prompt and decisive depletion we will very often save Nature the trouble of even expectoration—at least the inflammation may be frequently subdued before any expectoration appears, which will then be very trifling in quantity.

Again, Dr. Good takes no notice of antimony in the treatment of pneumonia, excepting as an occasional auxiliary to saline draughts in determining to the skin and promoting diaphoresis. "One of the most common, and, at the same time, most useful refrigerants, is nitre; which may be combined with the citrate of potash, or made to produce a more certain determination to the skin by the addition of camphor or of antimonial wine, or by a combination with the citrate or acetate of ammonia." This is all that is said of antimony as a powerful controller of the circulation, taken in doses just sufficient to produce nausea, especially when aided by digitalis, another important remedy which we do not observe in Dr. Good's methodus medendi.

Our author, however, strongly recommends the action of full vomiting, (by what medicine is not said) and that kept up for an hour or two, even when the pulmonic inflammation "has

made considerable advance." Not having given the measure a trial, it would not be fair to condemn it; but, we confess that, beyond nausea, we should be afraid to proceed in inflammation of such an organ as the lungs or their investment.

Our author notices the dangerous phenomenon of suppressed expectoration in the progress of pneumonia. We have seen it caused by an incautious cathartic, and we know of no medicine so powerful in restoring it, as the carbonate of ammonia in doses of ten grains frequently repeated.

We shall pass on to the subject of dysentery. It is always disagreeable to us to take up the review of systematic works, because analysis is not applicable to them, and criticism is the paramount duty of the reviewer in such cases. It is his main object, also, to point out what he conceives to be errors or imperfections, for the good both of the author and public; what he passes unnoticed being considered as having his approval. This is the path which we are unavoidably compelled to pursue in the work before us; and if our articles are chiefly composed of censures, it is because we cannot possibly even enumerate the chapters and sections of valuable matter diffused through these volumes.

The dissertation on dysentery not only disappointed, but almost provoked us. During the last memorable war, our fleets and armies were dispersed over every part of the globe, and our medical officers collected a mass of important information on the subject in question, such as had never before been collected or recorded. Yet, not a single writer of the last twenty or thirty years does our author seem to be acquainted with,—or, if acquainted with, has he noticed. Dr. Harty who, some fifteen or twenty years ago, made dysentery the theme of his inaugural dissertation at Edinburgh, is the only author quoted since the antiquated publication of Mosely some thirty years ago! Even Harty is only noticed as having accidentally fallen into our author's hands, after part of his chapter on dysentery was composed. In fact, it appears to us that Dr. Good, not liking to take the trouble of looking around him to see what was the existing state of our knowledge on the subject of dysentery, as scattered through all our periodicals and many distinct and excellent monographs during the last twenty years, has contentedly fallen back upon Sydenham, and determined "to take him as our polar star"—Sydenham who, it is our firm belief, never examined a dead body after he left his academical studies—at least he has given us no indication of pathological knowledge in any of his works!

We shall not enter into particular criticism on a chapter which, we are most reluctantly constrained to say, is unworthy of our author. In the etiology and pathology there is no infor-

mation at all—and what little is said of practice is, generally speaking, erroneous, bad, and antiquated. In another edition Dr. Good had better cancel the whole of this chapter, and consult those modern authors who saw and felt dysentery in various climates—who dissected their patients, and consequently became acquainted with the pathology of the disease—who discarded the unfounded Cullenian notion of contagion—who treated the malady with *success*, notwithstanding the dogma of Mosely to the contrary—and who, finally, have exhibited a more enlightened *ratio symptomatum* than his favourite Sydenham did, whose genius could only reach the sublime theory, that dysentery was "a fever turned in upon the guts."

The next subject discussed by Dr. Good is *bucnemia sparganosis*, or phlegmatia dolens. The only authors quoted are White, Trye, Ferriar, Hull, and Denman. He makes no allusion to the able investigation of Dr. Dickson of Clifton, in the second volume of the quarterly series of this journal; nor does he advert to the circumstance that phlegmatia dolens has occurred in the unimpregnated state, and even in the male. An instance of this last was met with in a marine by Dr. Denmark, then surgeon to Haslar Hospital, who has published the case and a plate of the limb—another instance happened in the person of Dr. Purdy of New York—and a third in the practice of the late Mr. Baynton of Bristol. Bating these deficiencies in the history of the disease, Dr. Good's account, though short, is sufficiently correct, and the therapeutic plan judicious. The latest information which we have been able to obtain respecting the pathology of the disease is, that it depends, sometimes at least, on inflammation of the great inguinal vein. A physician-accoucheur of this metropolis has some preparations, we believe, showing this circumstance, and means to publish on the subject.

Acute rheumatism occupies but seven pages of the volume under review, which is by far too short a space, when we consider the importance of the subject. Dr. Good has taken no notice of that very dreadful disease, rheumatic metastasis to the heart, which, for many years past, has occupied much the attention of the medical world.

In the chapter on gout we see nothing that need detain us. It also is rather short, when we consider what ponderous tomes have been written on the subject. Dr. Good, who appears to have had some visitations of the disease in his own person, seems not disinclined to a limited application of cold to the part arthritically inflamed; but acknowledges that he has never been able to muster up courage to put in execution that *striking* peculiarity of practice recommended, some years ago, by Dr. Balfour, namely, percussion and compression. "But our sheet-anchor," says he, "is opium; and it should be given freely,

and in union with some preparation of antimony, so as to act towards the surface generally, and thus restore to the living power its interrupted equilibrium."

On the subject of the exanthematous diseases, including plague, there is a very fair body of information brought forward by our author, but nothing novel, of course; we must therefore pass over this entirely, and here close our article, having already overstepped our boundary. In a future number we shall pursue our examination of these volumes.

ROCHOUX ON APOPLEXY.*

(From the Quarterly Journal of Foreign Medicine and Surgery.)

We are yet to learn whether APOPLEXY is the result of compression of some part of the brain; or whether effusions, and other causes of pressure, are merely the effects of pre-existing disease. Cases which, at least, strictly resembled apoplexy are recorded, in which no cause capable of producing compression was detected; and others are to be met with, where, without the symptoms of apoplexy, effusions, tumours, and other supposed causes of compression were found to exist. It may be observed, that, with the exception of hemorrhage, the causes of compression are seldom of a nature to warrant us in concluding that they take place suddenly. To ascertain the precise share which compression might have in causing apoplexy, M. Serres instituted experiments upon the lower animals, the result of which shows clearly, that, at least in them, apoplexy could not be produced from compression alone. (*Quart. Journ.* vol. iii. p. 271.) But these experiments having been made on animals in which the brain was previously in a perfectly sound state, we are scarcely warranted to extend the conclusions drawn from them to apoplexy in man; in whom there is strong reasons for believing that an unsound state of the brain precedes the accession of the apoplectic fit. An examination of the effects of compression from external causes, we think, may satisfy any one that something besides the mere compression is requisite to produce all the phenomena of spontaneous effusion of blood into the substance of the brain.

A slight investigation of the various causes which, from time to time, have been supposed capable of producing a stroke of apoplexy, shows clearly that most of these have no foundation in truth; or at least that their effects have been greatly overrated. Tumours in the brain are of slow growth; the soften-

* Dictionnaire de Médecine, tome ii. 8vo. Paris, 1822.

ing of its substance (*ramollissement du cerveau*), abscesses, and effusions of serum can only be considered as the slow and gradual results of inflammatory action; even extravasation of blood must be preceded by diminished power of resistance on the part of the vessels, or by a greatly increased force of the circulation in the head. It is true, we are told, that apoplexy is seldom preceded by premonitory symptoms; and on this head M. Rochoux says, that of sixty-three patients, whose histories he received, only nine exhibited precursory symptoms; and of these four were habitually subject to vertigo, which was not sensibly augmented immediately before the paroxysm. If we are to admit the whole of the preceding causes, we should expect that premonitory symptoms ought seldom to be absent. In a disease whose invasion is so sudden, the cause should also be sudden in its existence; or at least from being so slight as to give rise to no marked symptoms, to undergo a change capable of inducing the severe and instantaneous symptoms of apoplexy.

Upon this subject, the earlier writers, ignorant of anatomy, and the changes in structure which disease occasions, have advanced little except wild theory and crude conjecture. When anatomy began to be studied, new lights, it is true, were let in upon pathology, but the earlier anatomists were credulous in the extreme, often admitting as causes, many facts, which can be considered merely in the light of symptoms, or at least as exercising no influence whatever in causing apoplexy. Thus diseases of the heart, aorta, and other organs within the chest, and even many affections still more remote, have had an undue share of influence assigned to them in producing a shock of apoplexy. Enlargement of the heart, the *hypertrophie* of French authors, has more especially been considered as a predisposing cause; but even to this M. Rochoux attaches no value whatever; for out of forty-two cases of apoplexy that he inspected with the greatest care, only two had enlargement, or any disease of the heart! We see then that accuracy and truth, though of slow progress, are the sure results of anatomical research: most of these causes, with the endless list of useless and even superstitious remedies which were supposed to act upon them, have one by one been consigned "to the vault of all the Capulets." The division of apoplexies, however, into serous and sanguineous, was still a fruitful source of obscurity and confusion, and of many fatal errors in practice, of which the excellent work of M. Portal furnishes so many striking instances. But M. Rochoux is a decided enemy to this division: he confines the term apoplexy strictly to hemorrhage within the brain; a view of the subject which he claims the merit of being the first to have announced to the medical world. No one in this country, however, can be ignorant that many years ago Mr. J. Hunter,

in his inestimable work on inflammation, and in the evidence which he gave in the trial of Captain Donellan, for the murder of Sir Theodosius Broughton, took precisely the same view of the subject; though we are ready to confess that it has for the first time received an elaborate and systematic consideration by M. Rochoux.

To us it is apparent that effusion of serum, considered as a separate and uncombined affection, is not only a very distinct malady from hemorrhage in the brain, both as regards its nature and seat, but that the two can hardly ever manifest the same kind or order of symptoms. Effusion of blood, as a cause of apoplexy, is met with almost uniformly in the substance of the brain itself, where we can hardly suppose effusion of serum to take place—when blood is found in the ventricles, it may usually be traced to some ruptured vessels in the substance of the brain by means of a sinous communication. There could be no faith in diagnosis considered as a science, as is remarked by our author, if causes so totally different in their nature and situation were admitted as capable of producing the same train of symptoms. We cannot help thinking, therefore, that what is called serous apoplexy is really misnamed; and the evil effects of this division, in filling the minds of practitioners with undefined dread of debility, thus arresting their hand from the employment of our chief remedy, bleeding—free and decided bleeding, we believe, are manifest to all. Effusion of serum, says M. Rochoux, is one of the most frequent consequences of apoplexy, and therefore one of the causes which most frequently renders it fatal. The number of individuals who die of this effusion nearly equals the number of those who perish through the immediate consequences of the hemorrhage. It is difficult to perceive how the one affection so often produces the other, though not more difficult than to discover why an organic affection of the heart, for instance, or larger blood-vessels, should produce hydrothorax. It is enough for us to know that apoplexy has great influence in producing serous effusions into the ventricles.

Effusion of serum then into the ventricles is to be considered as quite inadequate to produce the sudden and total abolition of sense and motion, characterizing apoplexy. Besides this concomitant or subsequent effusion of serum, tumours, abscesses, softening, sometimes with, sometimes without, extravasation of blood are discovered—nay patients have died with all the symptoms of apoplexy, without exhibiting, on inspection, the slightest trace of morbid change. Are we to attribute the death to compression, in such cases, especially with the result of M. Serres' experiments in view, showing, in certain states of the brain, how little efficient compression actually is in giving rise

to the symptoms of apoplexy? Are we to consider with Mr. J. Hunter the hemorrhage capable of producing all the symptoms of apoplexy; or are we to suppose with M. Serres this hemorrhage to be merely an effect? We presume to think that the reasoning of Mr. Abernethy in many diseases applies strictly to apoplexy—the life of the part must have been originally affected, and we name the disease from its most prominent phenomena. But M. Rochoux believes that a morbid change of the brain, recognisable by the senses, at least, very frequently precedes the hemorrhage; and this change most frequently consists in a softening of a portion of the cerebral substance.

That precursory symptoms should not attend this affection, is not more wonderful than that tubercles of the lungs should often make great ravages, without disturbing the health or deranging materially any of the functions of the body. But of this *ramollisement* we shall speak more at length, when we take up the morbid changes of structure which accompany or are produced by hemorrhage into the brain.

Is there really then disease existing antecedent to effusion of blood in apoplexy? Is the hemorrhage merely an effect to be ranked with the other phenomena, effusion of serum, stertor, loss of sense and motion, &c.? We confess, notwithstanding the authority of M. Rochoux, we should hesitate to answer this question either way; and yet do we not know, that in a state of health the blood vessels are capable of resisting all the force which their own action, and that of the heart united, can bring to bear upon them? Many causes capable of producing increased determination of the blood to the head, such as mental affections, pregnancy, epilepsy, and strong impressions of cold, are said to bring on apoplexy; and yet when we attend to the prolonged efforts which jugglers and others make upon their heads, and how some people will allow an anvil to be placed upon their chest, or upon the region of the stomach, and allow it to be struck upon by hammers, and all this without producing any rupture of the blood-vessels in the brain, we shall hesitate before we allow that causes which simply increase the blood's motion towards the head, can produce apoplexy without some previous morbid change in the brain itself. M. Rochoux mentions the case of an inhabitant of Gaudaloupe, who died apparently apoplectic during the act of coitus, whom he inspected, and found an effusion of blood on the surface of the brain, but the brain itself was perfectly sound. If it be true what Mr. Rochoux says, that such causes precede apoplexy, in not more than a fifth of the cases which occur, we have still stronger reason to believe that there is pre-existing disease, in which case the hemorrhage in all probability would have occurred sooner or later of itself. On the other hand, if we attend to the structure

of the brain itself, a soft pulpy substance affording but little support to the coats of the blood vessels; a structure in short where it is conceivable that suddenly increased action, if in any part of the body, might tear the sides of the blood vessels or escape by the mouths of exhalants, opening in soft and yielding substance: if we attend to these facts, we shall at least be ready to grant that great facilities are afforded for hemorrhage taking place here, without the necessity of supposing that disease has rendered the vessels incapable of healthy resistance. The facts too of people dying of complaints not differing apparently from apoplexy, in whom no change of structure was detected, lessens the necessity for supposing a change of structure in the brain necessary for the production of the disease, and the total want of premonitory symptoms in so many cases; the occurrence in short of the disease in people in the enjoyment of the best health, notwithstanding the able reasoning of M. Rochoux, is not easily explained, upon the supposition that previous nervous, or any other nameless disorder existed. But upon these interesting points it is rather our wish to excite inquiry than to pronounce decisions, which, every one will be ready to admit, the facts on either side hardly authorize us to draw. If we attend to the fact, that precursory symptoms may in many cases have been overlooked, from the inability of the patient in severe cases to answer as to the point; and if we suppose farther, that in other cases they may have been so slight as hardly to attract the notice of the patient or his friends, so little proportionate, in short, in point of severity, to the sudden and overpowering symptoms of the accession, as to be forgotten or disregarded, one cause of the obscurity regarding the existence of previous disorder would be removed. Another source of want of precision on this subject arises from the unskilful and slovenly mode in which the brain is but too often inspected. May not morbid changes have been overlooked, or not properly appreciated, in many cases of apoplexy, in which it is roundly asserted that no morbid appearance could be detected? Such cases are but few, and not many of them recorded by men upon whose accuracy and judgment we can implicitly rely. M. Rochoux is of opinion that hemorrhage in the brain is necessary to produce all the symptoms of apoplexy; and Mr. John Hunter tell us he never inspected a case of apoplexy or hemiplegia, except one, in which he did not meet with extravasations of blood, and that one died of a gouty affection *resembling* apoplexy. When tumours and abscesses are found, on inspecting apoplectic patients, it is clear that they are to be regarded merely in the light of predisposing causes. If the disorder is purely nervous in the first instance, some affection of the vitality of the part, it is conceivable that serum may be effused in some cases, and blood in others, or that

they may be complicated; and it is still farther conceivable, that effusion of serum will more readily take place in debilitated subjects, effusion of blood in the robust. According to M. Rochoux, however, we are to consider these as constituting different diseases. He warns us also from supposing that slight causes can produce a true apoplexy; many having gone the length of attributing apoplexy to diseases within the abdomen, often exceedingly slight; and in this spirit, we find Weitbrectus, after having discovered three ounces of effused blood in the brain, and three or four small calculi in the gall bladder, seriously putting the question if these calculi could have caused the hemorrhage; and after having satisfied himself of the connexion of these as cause and effect, affectedly declining to trace the connexion very minutely!

The most prominent symptoms, and those most invariably present in a shock of apoplexy, may all be referred to obliteration, more or less complete, of the powers of sense and motion. The affection of the senses may vary from the slightest dulness, to the most profound stupor. The paralysis, or loss of the power of motion, is not less variable in extent and degree, sometimes affecting only one organ, at others abolishing totally all power of voluntary motion. These two affections, M. Rochoux supposes to be characteristic of the sanguineous apoplexy, though we should suppose that they cannot be considered as being perfectly diagnostic, as there can be no doubt that they occur from numerous other cerebral affections besides hemorrhage.

The pulse is exceedingly variable, being sometimes strong, full and hard, from which we have to fear a continuation or return of the hemorrhage; in others it is small and feeble, indicating want of powers in the constitution to recover itself; but in most cases the pulse undergoes no material change. The state of the respiration has by many been conceived to be diagnostic of apoplexy; but M. Rochoux says it is quite as variable as the pulse, being, like it, often unaffected, though it is generally stertorous; but the degree of stertor is far from indicating either the severity of the complaint or the strength of the patient. Stertor too is often present in other morbid affections of the brain besides apoplexy.

The appearance and colour of the countenance were supposed at one time to be quite characteristic of serous and sanguineous apoplexy, but the whole profession now knows how little trust is to be reposed on these appearances. The countenance may be pale, or it may be red, pale, green, yellow, livid, or of a deep violet; or it may be bloated, or the reverse. Independent of these, there is always a remarkable expression of stupor; and it is chiefly from an examination of the countenance that we ascertain the degree of this symptom. If the sphincter muscles

vened early, at the end of three days for example, it is black and in soft clots; but in proportion as death has been more distant, the effused blood acquires more consistency, and loses partly its black colour. If, in consequence of stimulating treatment, or of determination of blood to the head, there is a renewal of the hemorrhage, the blood first effused is found of sufficient consistency, surrounded by beds of the more recently effused blood, in a soft or semi-fluid state.

At the end of a month or six weeks the blood acquires still greater consistency, resembling that which is found in aneurisms. At a period still more remote from the attack, it is found still more solid, and of a pale red colour, approaching to yellow. In fine, it becomes entirely absorbed. Its quantity, in recent effusions, varies from a drachm or two, to four or six ounces—rarely more or less; and the severity of the symptoms almost constantly corresponds to the quantity of blood effused. A slight extravasation will occasion confusion of the senses only, without loss of consciousness; or it may produce blindness, palsy of the tongue, or lasting debility of one side of the body: in more considerable quantity it will occasion complete loss of sense with hemiplegia; and when still greater, the powers of sense and motion are completely abolished, and a speedy death usually ensues.

This effused blood is rarely met with except in the substance of the brain, where it is contained in cavities, which Wepfer and Morgagni have compared to aneurismal sacs, and these cavities not unusually communicate with the ventricles, or open by a rent upon the exterior of the brain. The walls of these cavities are very soft, strongly coloured red by the blood, of the thickness of a line or two, unequal, rough, and as if torn on their internal surfaces, and presenting many floating filaments when shaken in water. They are surrounded by a layer of cerebral substance of the thickness of two or three lines, of a pale yellow colour, soft, scarcely of more consistency than cream, and hardly misceable with water. The colour and softness of this cerebral layer diminish as it recedes from the apoplectic cavity, rendering it difficult often to determine the spot where the brain resumes its healthy appearances, and the morbid changes begin. Sometimes there is found, between the walls of the cavity and this external layer, another layer of a deeper yellow tint, always exceedingly soft, from two to four lines in thickness, and thickly beset with small bloody effusions not larger than pins' heads. If the blood has made its way to the surface of the brain externally, or to any point of the ventricles, it is usually mixed with portions of this yellow softened substance, which being retained by no obstacle, the blood carries along with it when effused. That side of the clots too, which

lies upon the rent, is mixed with portions of this softened substance. In these cases there is always a true loss of substance —a kind of erasion surrounded by a slight yellowish layer, which is soft and seldom thicker than the fourth of a line. This alteration of structure was first described by M. Rochoux, in 1814. He is at a loss to decide whether it takes place from the abode of the blood in the cavity, or precedes the hemorrhage, though he is inclined to the latter opinion; but to avoid all cause of discussion he calls this alteration of structure *concomitant softening* of the brain.

Such are the lesions of the brain in recent apoplexy; but they undergo afterwards very remarkable changes. The blood is after a time absorbed; the walls of the cavities cicatrize in some degree, or are slightly united by means of cellular and vascular bands which are circularly arranged, and between which is contained an ichorous liquid, reddish, more or less abundant, sometimes thick, yellowish, or gelatinous. These adhering walls are denser than the rest of the brain, of the thickness of a line or two, of a yellow or reddish tint, or sometimes of a rusty or deep brown colour. Sometimes they simply approach without being united by cellular or vascular adhesions, forming empty cavities. In one case dissected by M. Rochoux, the internal surface was as smooth as that of the ventricles, and moistened by a serous fluid. These cavities are constantly met with in apoplexy and the paralysis which succeeds to it, at whatever period of the disorder the patients may die, their number always corresponding to the number of attacks which the patient may have suffered.

Morgagni believed that hemorrhage of the brain occurred more frequently in the corpora striata and optic bed, than in all the other parts of the brain together. M. Rochoux gives us tabular views of forty-one cases, of which twenty-eight occurred in these situations. Hemorrhage appears to occur in both sides of the brain indifferently, although Morgagni has laboured to prove that the right side is more subject to it than the left.

The symptoms during life enable us to guess no nearer at the seat of the disorder than the side of the brain in which it may have taken place. Yet M. Serres, in a late publication, gives an account of cases of apoplexy in the cerebellum, which lead us to believe that apoplexy here is accompanied by an excited state of the genital organs. The facts however are as yet too scanty to enable us to draw any decided inference. M. Serres also believes that he can assign the part of the brain affected, which more particularly adduces paralysis of the heart, lungs, and commencement of the intestine canal. Such know-

ledge, even if attained, it is manifest, can be of little use to us in the treatment of the disease.

The most frequent complications with hemorrhage are softening of a portion of the cerebral substance, and effusion of serum, and one or other, or both of these affections may precede the effusion of blood, may be concomitant with it, or be found as its sequelæ. It appears that, by the French authors, this softening is attributed to slow chronic inflammation, and, if permitted to go on, ends in converting the affected portion of the brain into true pus. It is always found to exist along with an injected state of the vessels of the whole brain, less apparent in the tunics, and remote from the softened portion. This injected state of the vessels may be present in every possible degree, and when producing great distention of the vessels is apt to end in extravasation of blood. But if sudden and concentrated congestion takes place in the brain a real apoplexy is produced, and the effused blood, tending to keep up an inflammatory state of the organ, is apt to produce what M. Rochoux calls consecutive softening of a portion of the brain. It is obvious that there is a strong analogy between this softening and hemorrhage—that they are liable to be brought on by the same causes, mutually to produce each other, to give rise to the same symptoms, and to terminate in the same changes of structure. By this view a strong connexion is made out between inflammation of every kind within the skull, and extravasation of blood into the substance of the brain. As primitive and uncombined disorders, effusion of serum and softening, M. Rochoux says, may be readily distinguished from apoplexy; but when the diseases are in any way complicated together, the diagnosis becomes exceedingly uncertain. Considered as sequelæ of apoplexy, they do not render that disease more difficult of being distinguished; but to say which of the two it is, or whether there is only one or both occurring as sequelæ, we have no means of ascertaining. Effusion of serum, however, occurs in the great majority of cases, and is frequently free from complication with softening.

The softening, as an original complaint, manifests itself by a slow and comparatively gentle accession of its symptoms. It is apt, however, of a sudden to assume all the symptoms of apoplexy. More frequently the paralysis which it occasions, besides being slower in its approach, is accompanied by rigidity of the members, the muscles of the face are often spasmodically affected, the tongue is drawn towards the palsied side; but one chief means of distinguishing it from apoplexy, according to Rochoux, must be by an attentive consideration of the precursory symptoms. The mere existence of precursory symptoms is, in some degree, diagnostic of the presence of this morbid

softening. In proportion as all these affections of the brain are more severe, the less are we able, from the symptoms, to distinguish one from another.

Exhalation of blood into the ventricles is described by Morgagni and Baglivi, as very much resembling the true apoplexy, being sudden in its attack, and rapid in its progress; the chief symptoms being paralysis and loss of sense. In the same way apoplexy may be simulated by strong effusions into the cavity of the arachnoid externally. Exhalation of blood into the ventricles, M. Rochoux describes as resembling acute hydrocephalus, with the exception of the fever, which is not present; and the hemorrhage from vascular rupture of the arachnoid coat externally, is rapid in its progress, and bearing a strong analogy to hemorrhage in the interior of the cerebellum, or tuber annulare. It appears to us that much of what M. Rochoux advances, with a view to establish a diagnosis between many affections of the brain, is of very little utility, and leaves the subject much in the same state in which it was found. Thus, he says, that exhalation of the blood into the ventricles is not to be distinguished from hemorrhage into the cerebellum or tuber annulare; but that hemorrhage into the substance of the brain, when not *excessive*, may be distinguished from either. We doubt if these and many other distinctions, which he attempts to establish, can be made out during the life of the patient; and we are pretty certain that though they could, they are of no consequence whatever but to amuse speculatists in their closets.

With regard to the influence of age and temperament as predisposing causes of apoplexy, M. Rochoux has been at the trouble to make out tabular views of sixty-three cases, illustrative of these circumstances. Judging from these, it appears that from the age of twenty till sixty, the predisposition rapidly increases; that it is greatest from sixty to seventy, gradually diminishing beyond that age. There are few cases of hemorrhage in the brain in children; Rochoux knows of no well authenticated case, except one which is recorded by Serres; but even with regard to this case, M. Serres does not say whether it was not the complaint which he calls meningeal apoplexy, and others arachnitis. Temperament and seasons, according to our author, exercise very little influence in causing apoplexy; though he is not disposed to deny altogether the authority of Hollerius and Baglivi with regard to the influence of particular seasons. M. Rochoux believes that there is no external sign, appreciable by the senses, which indicates a disposition to apoplexy; and he refers to the observation of M. Corvisart, to show that individuals, of what are called the apoplectic make and temperament, are equally disposed to diseases of the heart. To hereditary disposition, our author is disposed to allow some share of influ-

ence, though he, at the same time, argues that this produces no particular form or temperament by which we can judge of the hereditary disposition.

Many authors have recommended the most absurd treatment in this complaint. Cœlius, for instance, says, that after having bled the patient till he has slightly recovered, he is to be placed on his back in bed, and there *jolted about;* and we are gravely advised to intermit the jolting occasionally, to allow the patient to recover from the fatigue it may have occasioned him. This plan is more lately advised by Hollerius and Jacquinus; but Forestus recommends substituting in its stead ligatures round the limbs. Sennert advised a remedy, if possible still more ridiculous, namely, that of holding a red hot iron so near the head as to warm, but not to burn it. Ætius, however, thought it right to apply the iron and produce an eschar. Many other absurd, and even hurtful remedies have been employed, or at least recommended, most of which were of a stimulating nature; but few medical people, we believe, require now to be told that stimulants cannot fail to be injurious; and as to the practice recommended by some learned doctor of Germany, not very long ago, of trepanning the skull to get at the effused fluids, we presume to think that he must be a rash surgeon indeed, who has the temerity to attempt any such fruitless operation. Emanuel Kœnig boasts of the surprising efficacy of a piece of executed criminal's bone, worn upon the paralytic limbs; which, however, is perhaps exceeded in absurdity by Nynam's remedy for palsy of the tongue, namely, three drops of blood drawn from the ear of a miller's ass, and given in a decoction of lentils!

M. Rochoux, we think very justly, criticises the mode of treatment which at present prevails in France. One, two, but never more than three small bleedings made at the foot, arm, or jugular, and leeches to the head or feet, are the first steps of the treatment. On the second day, an emetic or purgative is prescribed, and blisters are applied to the legs, thighs, and neck; afterwards stimulating and antispasmodic potions are prescribed, and occasional enemata to excite the action of the bowels. After some months, a course of mineral waters is recommended, and the paralytic limbs are treated with irritating liniments, electricity, and pumping water upon them.

If we attend to the injury which the brain has received, says M. Rochoux, and to the marked tendency to a continuation or recurrence of the hemorrhage, we shall see that many parts of the treatment commonly adopted are bad; and that our first indication ought to be to subdue the hemorrhage, the second to destroy the effort which tends to reproduce it, and the third to facilitate the absorption of the effused blood. If M. Rochoux

were not so afraid of one decided and free bleeding at the outset, and of purgative remedies, his directions otherwise do not differ materially from those of Mr. J. Hunter. We think, that, for emptying the vessels of the head, bleeding from the temporal artery, as recommended by Mr. Hunter, must possess a decided advantage over every other mode, in the estimation of any one who for a moment considers the distribution of the external carotid; and next to this, bleeding from the jugular vein. The injury which the brain has sustained from rupture of its vessels and laceration of its substance, must, on the recurrence of reaction, have a strong tendency to produce violent inflammation; and accordingly Rochoux remarks that the blood of the second, and still more that of the third bleeding presents the inflammatory coat. The patient ought always to be placed in a position with the head raised: we are apt to neglect this, on the supposition that the living body is little subject to the laws of physics. Having bled, according to Mr. Hunter, till the patient shows signs of recovery, we are to continue the flow of blood till he becomes faint; we are to give saline purgatives freely, to diminish impetus and promote absorption; we are to enjoin absolute rest, low diet; and to advise the patient to avoid coughing or sneezing; and never at any time are we to attempt to rouse, by stimulating treatment, the torpid members to action. Emetics, so freely used by many French practitioners, receive the unqualified reprobation of M. Rochoux; and indeed it is most unreasonable to subject to the violent action of vomiting, a man who has received a laceration of his brain. Speaking of the use of stimulants, Mr. Hunter says, we might with "equal propriety stimulate the fingers when their muscles were torn to pieces." A second hemorrhage, is often, says Rochoux, produced about the tenth or eleventh day, by the then more free employment of stimulants. In vain, he adds, do we stimulate; in vain do we tear the paralytic members; while the cause which deprived them of their nervous power remains, it is impossible their motions can be restored. In short, M. Rochoux recommends a strict antiphlogistic treatment; he has hardly any faith whatever in stimulants at any period of the disorder, but he strongly recommends those who will use them, to defer their use to a very late period of the complaint.

Many drugs, chiefly aromatics and stimulants, have been absurdly supposed to act as preventives against apoplexy; and Camearius recommends an amulet of coral to be worn: and adds, "long experience has convinced me of the efficacy of mace chewed fasting, twice a week, with a little argaric, in preventing apoplexy! M. Portal even gives a long list of remedies, which he says he has seen do much good in this way; but it is clear that in whichever way these remedies might act, they

could not possibly have any such effect upon the patient. We think we cannot do better than conclude by quoting the opinions of Abernethy. "We should pay attention, that the stomach digest thoroughly all the food that is put in it, that the residue be daily evacuated, that exercise should be taken in the open air short of fatigue; for without these we seek in vain for cures, or even preservation among medicines, and we but deceive ourselves by trusting to them."

Illustrations of the Inquiry respecting Tuberculous Diseases. By JOHN BARON, M. D. Physician to the General Infirmary at Gloucester. London, 1822.

(From the Edinburgh Medical and Surgical Journal for April, 1823.)

No department of medical science is attracting more attention, or cultivated with greater zeal, than that of pathology; and no inquiry of pathology seems to be pursued with so much interest, as that which illustrates the nature of those organic changes, which give rise to the formation of what are named consumptive diseases. Within the compass of a few years, we have had occasion to notice the researches of M. Bayle, and the original views of Dr. Baron, and to be the medium of communication for the valuable observations of our correspondent Dr. Abercrombie; not many months have elapsed since we gave a pretty full detail of the accurate researches and interesting discoveries of M. Laennec; and before us at present is another treatise from Dr. Baron, intended to illustrate the opinions published in his Enquiry, and to elucidate and confirm his doctrines on the formation of those tubercular disorganizations, whether occurring in the lung, or the intestines, or in other tissues, which too frequently terminate in the unremediable destruction of the part and the extinction of life.

The former work of Dr. Baron showed, that various observations and experiments, chiefly on the lower animals, led him to be dissatisfied with the usual pathological opinions on the origin and nature of tuberculous diseases; and to form on this subject inferences, which, so far as we know, are peculiar to his illustrious and lamented instructor, Dr. Jenner and himself. The inferences to which we allude may be referred to two general heads; 1*st*. Those which relate to the origin of tubercles, to the material cause of their production, or the organic tissue or system which is the chief agent in their formation; 2*d*. Those which relate to the mode in which the tubercles are developed or generated, to the course which they follow, and, in short, every thing connected with their progress as pathological bodies.

The former of these subjects, which was discussed at some length in his Enquiry, is almost entirely excluded from the present volume, or mentioned only incidentally; and the principal object of the author in the work before us, is to give a full and deliberate examination of the latter point, a minute account of the progressive changes which tubercles undergo, a statement, historical and critical, of the descriptions and opinions of various authors, ancient and modern, and observations on the measures, which the principles of Dr. Baron would lead to consider most effectual in resisting and combating the organic injury which these parasitical bodies occasion.

That the reader may form a more distinct and precise idea of his inferences on this point, and may be enabled to pursue the train of investigation more easily, Dr. Baron condenses the whole of them into the following series of independent propositions.

"First, then, I affirm, "That tubercles exist in almost every texture of the body, and that their origin and essential character will probably be found to be the same, wherever they are discovered.

"II. That tubercles, in their commencement, are small vesicular bodies (*i. e.* hydatids) with fluid contents.

"III. That these bodies subsequently undergo transformations, on the nature of which their tuberculous character depends; that these transformations are progressive, but not uniform; and that it is only in the larger bodies of this kind that they can be accurately traced. That they commence with an opaque spot, which advances with different degrees of rapidity, and ultimately converts both the contained and containing parts into substances very different from what they were at first.

"IV. That on the size and relative position and structure of the tubercles, which are thus formed, depend the characters of many of the most formidable disorganizations to which the human body is exposed.

"V. That considering the transmutations which these bodies undergo, the condition in which they may be found will be modified by the time at which they may happen to be examined.

"VI. That it is rarely that we can have an opportunity of seeing the first steps of these morbid phenomena in the human subject, because the tubercles are generally formed, and the alimentary character of course lost, before death permits us to make inquiries respecting altered or morbid structure.

"VII. That some tumours are formed by the aggregation of tubercles, and that the characters of such bodies are materially influenced by the relative position and contents of the elementary parts, of which they may happen to have been composed, or, in other words, that 'varieties in the arrangement of the elementary parts of morbid growths, will of course cause corresponding varieties in their appearance.'

"VIII. That, therefore, diversity of appearance in tubercles or tu-

mours does not imply diversity of origin, for it has been demonstrated that substances and textures of very different properties may be found even within the same cyst, thereby merely denoting different gradations in the changes to which these bodies are liable.

"IX. That the disorganizations above referred to are not the product of any species of inflammation, and that though inflammation may attend their growth, and modify the symptoms which they occasion, yet that it is very different both in its origin and consequences from that species which attacks a part unaltered by previous disease; that, in the first instance, it is to be considered as the consequence, and in the latter as the cause, of altered texture." pp. 4—7.

Of these propositions, some are essential and peculiar to the doctrines of our author; others are only accessory, and common to him with most other pathologists. It is chiefly to the essential (1, 2, 7 and 9) that it is profitable either for us, or for our readers to give our present attention.

Dr. Baron requires no more of his reader than simply to understand well the meaning of these propositions, and to divest his mind of every prejudice or bias. His assent to their truth, or rejection of them as inadmissible, must depend entirely on the evidence by which they are supported.

It is easy to see that this will neither be a light labour, nor a doctrine which is likely to acquire many proselytes in a short time. The evidence which is requisite to prove that tubercles in any of the tissues, and pulmonary tubercles of course, are in every instance originally minute vesicular bodies, is at once, as our author has acknowledged, (propt. IV.) very difficult to obtain, and beset, at the same time, with several sources of mistake; and although this were established, by what means are we to satisfy ourselves that these vesicular bodies are actual hydatids? The body or the animal, whichever you choose, to which medical men have agreed to apply the term *hydatid*, resembles, neither in its appearance nor properties, at any time of its existence, the pathological objects, which have been named *tubercles*. Without, however, dwelling on such objections at the very outset, we must inform our readers, that Dr. Baron proceeds to establish the propositions already quoted, by evidence derived from the following sources; 1*st*. By the history of the formation and progress of tubercles in the human subject; 2*d*. By the history of the formation and progress of tuberculous growths in the inferior animals; 3*d*. By a reference to the observation and opinion of Hippocrates among the ancients, and Fernel, Sennert, &c. among the moderns; 4*th*. By showing that the pathological descriptions given by modern observers are founded on erroneous or biassed observation, and, if carefully examined and purified from the sources of error, would tend to establish the pathological principles, which he has delivered on the nature and origin of tuberculous diseases.

It is certainly undeniable, as our author has remarked, that pathological writers have manifested great discrepancy of language in their descriptions of the varieties and stages of pulmonary tubercle; and we had occasion to notice, not long ago, in what manner the miliary tubercle of M. Bayle is to be understood. These discrepancies, our author thinks, his description will tend to reconcile.

"When tubercles are first formed in the lungs, they are not cognizable by the touch, by reason of the delicacy and elasticity of their structure; but they are visible on careful inspection. They are very small vesicular transparent bodies, and shine amid the unchanged texture of the surrounding lung. Should any of them happen to have been generated on the surface of the membranes, they there may be seen clustering together, and resemble, both in size and general character, the beautiful globular incrustations, which beset the stalks and leaves of the ice-plant. In the human subject it is *very rarely* that we can have an opportunity of detecting them in this, their primary state; and consequently most of the descriptions which have been given, commence at a period somewhat later in their progress. At that time the softness and delicacy of the vesicle are lost, its transparency is diminished, and its size is increased. On examining the lung, where they may exist, by the touch, a distinct granular sensation is communicated to the fingers. The progress from this period is evinced by an augmented size, a firmer texture, and a complete loss of transparency, a yellow opaque body being perceptible. In this state they sometimes fall into ulceration, and prove fatal. But before such an event takes place, it occasionally happens that many of them advance further, and exhibit other appearances. Except where they are in contact with each other, they go on increasing in bulk. The coats of some become thick and hard and almost cartilaginous, while their contents may vary both in colour and consistence. Others proceed in a different way, and are condensed into solid bodies of an uniform texture,the cysts and the containing parts being scarcely discernible from each other.

"The appearance then of the lungs of those who die in this state is as follows: some tubercles, when cut through, will be found to be firm and solid, others with thick dense coats containing curdy, cheesy, or purulent-looking substances; others will be found to have been in part destroyed by the progress of the ulceration, and to show the firm and almost cartilaginous remnant of the emptied cyst, conspicuous amid the surrounding disease. Should a greater number of contiguous tubercles have fallen into this state, deep and extensive and irregular shaped fissures and excavations are thereby formed.

"In the progress of the tuberculous disease, there are corresponding changes in the surrounding lung, which it is necessary now to note. At the first development of tubercles, whether in the lungs or elsewhere, the surrounding texture seems to undergo little or no alteration. The lung retains its fresh pink colour, and its light elastic feel, and there appears to have been no interruption either to the circulation of the blood or air.

"As the tubercles increase in size and in density, and approximate each other, they cause greater disturbance in the system. The blood is impeded in its circulation, and respiration is of course rendered quick and laborious on slight exertions. The consequences are obvious; the lung becomes firmer, and of a darker colour, and ultimately exhibits that appearance, which has been supposed to be indicative of a particular species of disease.* Whether this be an idiopathic affection or not, it is not at present material to inquire; as, in such examples as we have described, it is manifestly occasioned by the growth of foreign bodies in the lungs; and the darkness of its colour and its induration increase in proportion to the obstruction which is offered to its functions.

"The dark and indurated lung, which occasionally surrounds the tubercles, may, under the circumstances which I am about to describe be obliterated; this takes place when the tubercles increase in size and coalesce, a dense solid structure being thus formed, with here and there partial traces of the original tuberculous character, to the total exclusion of every thing like the pulmonic texture." pp. 10—13.

Though the symptoms indicating, or believed to indicate these pathological changes must be generally and well known, yet a regard to candour and correctness obliges us to give Dr. B.'s semeiography.

"Tubercles, in their incipient state may exist without producing much disturbance in the system, and they may pass onwards towards consolidation, if they be not very numerous, without affording almost any signs of their existence; and in this consolidated state they may continue, and not in any material degree tend to abridge life. The unexpected occurrence of solid tubercles or tumours in the lungs of those, who had not previously manifested any symptoms of such disease, bears me out in this assertion. When tubercles are fully consolidated, there is the strongest reason to believe that they do not subsequently fall into a state of suppuration. This occurs chiefly in those that were not destined to arrive at this point.

"The consolidation therefore just referred to, may in some measure be considered as a favourable termination to tubercle, as life has been found to be compatible with their existence, except in cases where they occupied a large proportion of the lung, or produced accretion of the membranes. It is in that period of their progress, which is intermediate between the state last mentioned and their first development, that all the symptoms characteristic of tuberculous phthisis occur. This will be apparent by attending briefly to the ordinary progress of the disease.

"In a person who has tubercles, a frequent cough without any expectoration, but with occasional oppression about the chest, and hurried respiration on slight exertion, may exist at intervals for many months, or even a longer period, without any other sign of disease. What is commonly called a fresh cold, may increase these symptoms and render them more permanent; and then the patient, who never

* Hepatization.

expectorated before, may perhaps be surprised by spitting up a yellowish or whitish globular-shaped mass, tinged with blood, or a gush of blood may precede an occurrence of this kind. I have known the last mentioned symptom repeatedly happen to a most alarming extent, in a case where there was great destruction of the pulmonary tissue by the consolidation of tubercles, but where, though the case proved fatal, there was never any expectoration of the matter from tubercles. It is from this and other kindred cases, that I infer that tubercles once consolidated do not subsequently suppurate or ulcerate.

"Such an expectoration, as I have above described, is a very sure token of tuberculous disease. One of them has given forth its contents, and it plainly tells us that there may be more in a condition likely to do the same thing. In proportion to the number of these bodies, and the rapidity with which their texture is broken down, is the progress of the disease." pp. 14—16.

On the pathognomonic character of purulent expectoration, we are presented with an observation, of the truth of which we have been long convinced, and which shows how useless this much laboured diagnostic has now become in consequence of our more accurate pathological knowledge.

"The appearance and the quantity of the matter expectorated differ much at different periods in the same case; that which is discharged from a tubercle, strictly so called, varying from that which may be excreted from the disease of the mucous surfaces, which has been excited by the tuberculous affection. The appearance of pus, by no means, as is generally supposed, necessarily indicates the presence of tubercles, for the contents of these bodies are very often far from being purulent. It is manifest, therefore, that this test, which has been looked for to determine the existence of tuberculous disease, may be fallacious. The reader must remember, that I am here speaking of the matter which is contained in the tubercle itself, in contradistinction to that which is yielded by the surfaces of tubercles that have ulcerated and discharged their contents, as well as of that which is afforded by the diseased condition of the surrounding parts." pp. 17, 18.

We are indeed quite satisfied that daily observation presents us with numerous consumptive cases, which terminate fatally without a particle of purulent matter being expectorated; and equally certain, that when purulent or puriform matter is expectorated, it does not prove that the tubercles are softening, or that the case will certainly prove fatal.

This account of the progress of the pulmonary tubercle is followed by two cases in confirmation of the truth of the notion, that tubercles are originally minute vesicles or hydatids. The first is certainly the most conclusive, as the transparent vesicles were found both on the pleura, and in the pulmonic cellular tis-

sue, and as various parts of the lung presented them in different states of maturation, according to the conceptions of Dr. Baron. With the second case, we confess ourselves somewhat perplexed, as we cannot perceive, that it tends in any way to establish the object of the author; but assuredly proves one assertion, in which we quite agree with him, that very considerable disorganization of the lung may have taken place without being distinctly manifested by any unequivocal external signs. Dr. Baron concludes, in general, from such cases, that very great changes have taken place previous to death; and that the appearances which dissection discloses are very seldom those which existed at the commencement of the disease. He avails himself of this imperfection of our art to render doubtful all the descriptions which have been hitherto given of tubercles; and charges the pathologists of the present day with believing, that all disorganizations exist from the beginning in the state in which they are found at death. Most pathological writers have distinguished tubercles according to the organ in which they occur; and have described one variety of tubercle as existing in the lung and another in the liver or mesentery. Dr. Baron disapproves of this distinction as inconsistent with the uniformity of the general laws which regulate the origin and formation of tubercles, and not confirmed by what is found in nature. The first reason we look upon as a complete *petitio principii*, and therefore not deserving any attention. In support of the second, Dr. Baron adduces two cases; the first of which certainly shows, that the liver may contain tubercles or tubercular bodies in various states of softening or maturation, and with various effects on the surrounding texture of the organ; but we doubt whether hydatids or the hydatidical character could have been recognised by the eyes of any other observer except Dr. Baron. The second is much more conclusive; and furnishes an example of tuberculous disease of the peritonaeum, mesocolon, spleen, lungs, and pleura. The mesocolon presented many semitransparent tubercles, little advanced beyond the vesicular or hydatidical character. Some, on the other hand, were completely transformed into solid bodies; while others had contents of a curdy or cheesy consistence, easily squeezed from the cyst. The surface of the spleen presented a great number of minute hydatids,—some yellow, and as large as millet seeds; the greater number small, and, when seen through a glass, of a bright sparkling appearance. A few tubercles, about the size of millet seed, were found in the right lobes of the lungs, and on the middle lobe of the left side, where it extends over the pericardium, was a solid oblong tubercle, about the size of a grain of wheat. Other portions of the same membrane presented similar tubercles, some of which had attained the size of hazel nuts.

The second chapter of our author's work, which treats of the tuberculous diseases of the inferior animals, and the evidence which they furnish in support of his opinions, is very interesting. The information which it contains is chiefly extracted from the work of M. Dupuy, the veterinary surgeon and professor at Alfort, and relates to diseases in the horse, ox, and sheep. And it appears very clearly, that, although M. Dupuy had not been struck with the hydatid origin of tubercles, or any other connexion between these bodies, yet he had very often observed in the same animal cysts containing hydatids and cysts containing tubercles.

Thus, "in a cow, six years of age, killed on the 2d of February, 1819, the pulmonary tissue was very much altered. It contained many cysts, enclosing hydatids of different magnitudes, from the size of a pea up to that of a goose's egg. Other cysts, of which the coats were of the consistence of cartilage, and even osseous, were filled with a substance analogous to that of bone; those which enclosed the hydatids were smooth, and had the appearance of a mucous membrane. We thus found in these lungs hydatids and tuberculous matter, which would seem to prove, that these bodies, though very different in their physical qualities and their organization, have many affinities in common in regard to the causes which determine their formation, and the manner in which these bodies alter the pulmonary tissue." pp. 46, 47.

And in a cow affected with tubercular phthisis, tuberculous matter (*matière platreuse*) of a yellow colour was deposited in the pulmonary tissue in

"Rounded tumours of different sizes, with unequal surfaces, irregular and rough. Some were uniform, others were of the size of an apple, from which doubtless arises the name of *pommeliere*, which the vulgar give to this malady. These tumours were throughout covered by the pleura, which was not sensibly altered. The lymphatic glands, situated at the division of the bronchia, contained also in their tissue tuberculous matter; their primitive organization had disappeared, and these ganglions had undergone a tuberculous disorganization. Another object, which merits the attention of observers, is the presence of a great number of hydatids of different sizes in the parenchyma of the lungs. *These hydatids, regarded and described by zoologists as organized and living bodies, might throw some light on the origin and formation of tubercles, or at least prove, that these bodies, which disorganize the lungs in the same manner, develope themselves under similar circumstances. This connexion may become of much value in the etiology of those diseases, which deeply change the organization, and which are so common and so fatal.* pp. 48, 49.

In the conclusion of the chapter which relates to Farcy, the author's opinions on the agency of the lymphatic system in producing tubercles, are more distinctly brought into view; and

he deems this disease to proceed from a morbid state of the lymph of the lymphatics of the limb.

The third chapter is of a very miscellaneous nature. Its chief object is to collect evidence in favour of our author's views from the ancient physicians; but several other purposes are also served by this consultation of the ancients. In the *first* place, Auenbrugger and Corvisart are deprived of the merit of discovering the diagnostic use of thoracic percussion; and even the glory of Laennec in contriving auscultation and inventing the stethoscope, is somewhat obscured. In the *second* place, Dr. Baron shows very satisfactorily, as far as the subject admits, that Hippocrates had very clear notions on various diseases of the chest, and was aware of the distinction between tuberculous affections, and peripneumony or other inflammatory disorders of the lung. Our author further shows, that Hippocrates looked on tubercles as a kind of hydatids, and that he formed this conclusion from observations which he had made on the diseased lung of the ox, the dog, and the hog. From the physician of the Archipelago, Dr. Baron is constrained to take an immense step down to Fernel and Sennert, who seem to have had the same opinion. The passage which he quotes to prove that our countryman Morton thought so, makes mention of a glandular tumour only, and cannot be deemed evidence of any consequence to the question. After him, this connexion between tubercles and hydatids is noticed only in one or two passages of Morgagni, who appears to Dr. Baron to have indulged so much in speculation on the examples which had fallen under his knowledge, that he had overlooked entirely the manner in which hydatids become tubercles, and, consequently, did not anticipate the light, which our author was destined to throw on the pathology of consumption.

The fourth and fifth chapters are very long, and very critical. They contain a minute examination of the labours of Bayle, Laennec, Broussais, and Abercrombie; and enter very fully into the researches of these authors on consumptive diseases, and the descriptions which they have given of tuberculated or otherwise disorganized lungs. It is difficult to conceive or to communicate a very just idea of the merit and nature of this disquisition. To us it appears to have led the ingenious author into the commission of two errors, equally injurious to his cause, and from which every scientific inquiry, and, above all, every medical investigation, ought to be completely free. We allude, in the *first* place, to the extraordinary degree of confidence which the author reposes in the truth and correctness of his own pathological princples; and, *secondly*, to the spirit of contempt and the tone of asperity in which, in complete consistence with this, he animadverts on those of all other

writers. Our readers must have already discovered the peculiar fondness which our author manifests for drawing comprehensive and general conclusions, from premises which, if not very partial, have not been hitherto shown to be very uniform in their nature or application;—the singular eagerness with which he grasps on a few scattered facts, floating like the drowning mariners of Orontes,* in the mighty sea of medical observation, and, after moulding them by the force of his own reasoning, to the fashion of his theory, holds up the delineation to the attention of the profession, as a true representation of the uniform and invariable course of natural phenomena. This erring logic, however, is a very trivial sin compared with the manner in which Dr. Baron has canvassed the researches and opinions of his fellow-labourers in the work of pathology, and contrasted them with the merit of those, which he himself professes and defends. Every conclusion which is at variance with his own, is unfounded, unfairly deduced, or in some manner rendered worthy of censure; but whenever the facts adduced by others, are too strong to be positively set aside, or correspond in any distant manner with the pathology of our author, they are ingeniously applied to illustrate and confirm the truth of his doctrines.

It is quite certain, that whoever has embraced a particular mode of thinking or observing, or whoever has caught what he deems an explaining glance of any ambiguous and complicated subject, will see every object in the particular light, in which the habits of his mind have led him to look, and will, in the true spirit of generalization, bend every fact and every opinion to his own peculiar views. Whatever suits not this exact measure which he has framed, must be incorrect, or has been misrepresented;—whatever suits it, but is misplaced, must be adjusted or explained;—and the few points of resemblance, which the keen and practised eye seldom fails to perceive, are eagerly seized, and dexterously applied, to speak a language of which their authors thought not, and express views, which were seen by none but the generalizing observer. We do not say this in express terms of Dr. Baron; but we wish it were possible to rescue completely his observations on the works of the pathologists we have mentioned, from this character. It is not our intention to enter into an examination of the merit of these remarks, for we do not choose to criticise a criticism; much less is it our purpose to write any defence of the pathological principles of these authors, who are quite competent to defend themselves, and to confirm the correctness of their observations against the arguments adduced by our author. If further ob-

* Apparent rari nantes in gurgite vasto.

servation shall show their descriptions to be inaccurate, we are certain that the love of truth will cause them to acknowledge the mistake, and that their candour will at once prompt them to express their gratitude to Dr. Baron, for the opportunity which he has given them of acquiring information and correcting error. If, however, the theory of Dr. Baron shall prove to be, as we think it may, a partial doctrine, which applies only in some cases, we cannot perceive the propriety of the very decided language in which he has animadverted, especially on the statements of M. Laennec and those of Dr. Abercrombie. A very brief attention to this part of our author's observations, will enable our readers to judge for themselves.

The great point on which Dr. Baron is at variance with M. Laennec, relates to the manner in which tubercles proceed to undergo their successive changes. M. Laennec, it will be remembered, agrees with all the pathologists whom we know, in representing the firm state of the pulmonary tubercle as preceding its softened or semifluid condition, and in viewing the depositesof semifluid matter, commonly named *vomicæ*, as the result of the softening of one or more tuberculous masses. The views which Dr. Baron has formed of the progress of these bodies, lead to a conclusion quite opposite; and he contends most strenuously, that the tubercle originally contains transparent fluid, which afterwards undergoes various changes in consistence, and finally terminates in complete consolidation. A necessary consequence of this view, to which Dr. Baron consistently enough adheres, is to represent the *vomica* as the intermediate condition between the nascent or incipient tubercle, or the semifluid vesicle, and the firm and condensed body to which all anatomists have hitherto agreed to apply the term "tubercle." Now, it is easy to see, that the whole of this difference resolves itself into a matter-of-fact question,—an *observatio crucis*, as it might be named,—to determine whether the softened state actually precedes the firm solid condition, or, whether this firm condition is not eventually succeeded by another and a second softening process. Examinations of tuberculous lungs have shown tubercles small and large, transparent, half-transparent, and opaque, soft and firm, semifluid and solid; but the difficulty is, to determine which of the two states precedes the other. Pathologists and morbid anatomists have generally hitherto deemed it most rational, or, at least, most consistent with the course of phenomena remarked in pathological processes, to consider the firm and consistent state as the original and early one of the tubercle, and to deem the softened or fluid condition an ulterior and subsequent one, which was ascribed universally, till the time of M. Laennec, to an inflammatory and suppurative process, to which it was conceived these bodies tended. The view

which M. Laennec and some other cotemporary observers gave of this subject, differed merely in the abstraction of the inflammatory action, for the softening (*ramollissement*) is indeed only a particular mode of the formation of matter. We will acknowledge, that this description appeared to us, as to most others, certainly not made from fancy, but copied from what is seen in the lungs of phthisical subjects. It is indeed true, that the appearances observed in such cases are seldom so complete, as to exhibit an entire and connected series of the successive changes, which may take place in parasitical tumours, from the first moment of their origin to the time when they have been consumed by their self-destroying action. All that the pathologist can see and can reason from, are generally the appearances which are left after the organ has been so much destroyed as to be unfit for the functions of life, or those changes which the accidental occurrence of another disease may present;—and this only in cases of which little is known or marked, unless absence of symptoms of pulmonary disease. The conclusions, therefore, which M. Laennec has drawn, in common with other pathologists, from these appearances, are merely what might be expected in the circumstances; and we doubt whether more could be said of those which Dr. Baron has formed, although we should admit them to be perfectly legitimate.

Against Dr. Abercrombie our author states objections not unlike these, and opposes particularly the mode of considering the induration of pulmonary tissue, either from tubercular growth, or from other causes, as the cause of consumption, or rather the state previous to the destructive process, on which the consumptive symptoms depend. Dr. Baron considers this induration as the effect, and not the cause, of the pathological changes which occur in the tuberculous lung. He never seems to think, that neither his own opinion, nor the one which he combats, is universally true; and that it is possible, that, in one set of consumptive cases, the pulmonary disorganization may be preceded by induration, and, in another, it may be followed, or at least accompanied throughout its course, by this circumstance. Another reason for which our author disapproves of Dr. Abercrombie's pathology, is, that he represents inflammation, chiefly chronic, as the cause of organic changes, while Dr. Baron looks on this process as the invariable effect of these bodies. Various circumstances prevent us from entering into the merits of this question. We cannot answer for the manner in which our author understands the descriptions or reasonings of Dr. Abercrombie, and we are unwilling to attempt any defence of principles, of which we ourselves may have only an imperfect understanding. But it is quite certain, that inflammation, or an inflammatory action, acute or chronic, does accompany, from

the beginning, some of the most serious disorganizing processes incident to the animal tissues; and it is equally certain, that it is very difficult for the most accurate observer, or the most acute pathologist, to determine whether this process is a *cause*, or only a *part*, of the action in which the organic change consists. We believe that the most intelligent pathologists of the present day, and we know that Dr. Abercrombie is one of this order, look on the progress of the pulmonary tubercle as a process quite different from inflammation, though it is much influenced by this action in the contiguous tissues; and further think, that this and other analogous processes are very often attended with chronic or occasional acute inflammation in the organic substance immediately connected with it. Such inflammatory action is certainly, in general, to be looked on as the effect of the previous disorganization, and of the morbid process which is at the moment going on; and Dr. Baron appears to us to be combating a phantom of his own creation, when he hopes to convince pathologists that this is the case. We must not forget to observe, that much of this discussion is gratuitous, in so much as it does not relate directly to the point under examination, and as Dr. Baron has chosen the pathological observations of Dr. Abercrombie on diseases of the brain and its membranes, as the object of his animadversions on this subject.

Such, very nearly, is the kind of evidence, and the mode of reasoning by which Dr. Baron has attempted to prove the truth of his opinions on the progress and pathological changes of the pulmonary tubercle. A desire to give a candid and just view of these, has led us to be more full in our extracts, and more minute in our analysis, than we could have otherwise justified ourselves in being, and has prevented us, as much as may be, from delivering any opinion on the merits of Dr. Baron's cause, in the course of our account of his reasonings. It will appear that he has spoken with sufficient severity of the errors, which he conceives other pathologists have committed; and that he has expressed himself with sufficient confidence that they are in the wrong; and we are willing to think, that, in many points, he is correct. We wish we could say that he has been equally successful in establishing his own doctrines, and that he had adduced undeniable and conclusive evidence that tubercles are, as he represents them, hydatids;—that their soft and fluid state invariably precedes the indurated;—and that a pulmonary vomica (we will avoid the term abscess) is truly and actually a hydatid, proceeding onward from the fluid and soft to the solid and consistent state. We fear, however, that this point will not appear so certainly established to his readers, as it evidently is to Dr. Baron; and we think that, unless he can adduce more direct and unquestionable arguments,—evidence, in fact, which

without appealing to the livers of rabbits, or the varying and opposite appearances sometimes presented in the lungs of the glandered horse, will show that tubercles begin in the human lung in the form of minute vesicles, proselytes will neither be soon nor numerously gained to his system. It is likewise evident, that the arguments which Dr. Baron employs to prove the progressive changes stated by him to occur in the tubercle, from a transparent or semitransparent to an opaque fluid, and thence to a state of consistence and firmness, or even induration, are by no means sufficiently conclusive or direct to establish the point. We have a singular aversion to the quotation of authority, unless it be of the first order, and unless it is impossible to make out our case without such a resource. We have, above all, an aversion to the quotation of the authority of Lord Bacon in all instances, not because we think his authority insufficient or improper, or not of the most excellent kind;—but because we generally observe, that it is the frequent resort of a weak or defenceless cause, and that many authors seem to believe that the bare mention of Lord Bacon's name is sufficient to strengthen the most impotent conclusions, and to prove the most untenable positions. Wherever the name of Lord Bacon is much repeated, or the word 'induction' often introduced, we have generally been able to recognise a singular weakness in the chain of reasoning, and an obvious want of connexion in the links of which it consists. We had much rather see a good piece of inductive reasoning, as it is named, or, as we would call it, a good specimen of analytic investigation, without an allusion the most remote to the great Verulam, or any sign that the author was aware of the existence of such an eminent person, than a vague and declamatory disquisition, with strong inferences from a few instances, numerous analogical arguments, and more numerous quotations from the *Novum Organum*. It is quite as easy to examine a subject well, and to sift the conflicting or discordant parts, so as to elicit the true statement of their relation, and to exhibit a connected train of argument founded on facts which are properly arranged, as to take so much pains to prove that the author has read or consulted the works of Lord Bacon, or knows that he suggested a method of reasoning, which academicains have termed induction. We will not say that the whole of this character applies to the work of our author, for we must do him the justice to say, that he is evidently well acquainted with the principles of the Baconian philosophy; but we must confess, that the previous observations have been irresistibly and involuntarily suggested to us by the perusal of his work. We will not, however, embarrass him, either with the inductive method, or quotations from the *Novum Organum*, but merely state a few simple observations on the evidence, which we conceive requi-

site to establish his doctrine, and on the slender points in his argumentation.

1. We begin with observing, that our author's position, that tubercles commence in the form of minute vesicles, is by far the most probable part of his doctrine. It is very difficult, indeed, to prove it directly, or to give the sort of evidence which the rational physician will require that this is the case. For, in the *first* place, it is rare that we meet with opportunities of examining pulmonary tubercles in that early and nascent condition which can alone furnish the information we require. We, indeed, occasionally meet, in the lungs of persons who have died from accident, or have been cut off by more acute maladies, considerable variety of tubercular disorganization, which the symptoms would have scarcely led us to suspect; and we certainly find, in such lungs, all those varieties which have been believed by anatomical observers to depend on the different stages of progress, in which these bodies were. But, *secondly*, how can we seize on the exact case, or the precise period in that case, at which we shall find the vesicular appearances described by Dr. Baron, asserted by him to be the embryo tubercle? Are we quite certain that these bodies would actually become the opaque, dense, grey tubercle, which produces such mischief in the pulmonic tissue? So far, however, as the doctrine is susceptible of direct proof, it may be deemed likely enough, insomuch as it accords in general with the observations of MM. Bayle and Laennec. The latter pathologist has shown, that the miliary tubercle of M. Bayle is the nascent or embryo state of the common pulmonary tubercle; and we are not unwilling to admit, that the varying descriptions of grey, transparent, millet-seed tubercles, which have been given by these pathologists, are most easily reconciled upon the statement given by Dr. Baron. This part, therefore, of our author's doctrine ought to be attentively investigated, and the means of confirming or refuting it, diligently sought, as by far the most probable, and, perhaps, the most important of his researches.

2. Although, however, we admit the vesicular character of the minute or nascent tubercle, we are instantly met by another difficulty, which is by no means so easily removed. What are the proofs that this vesicle is a hydatid? Does it resemble this production in its appearance and properties, or in the ulterior changes which it is destined to undergo? Has any one seen hydatids of so small a size as the nascent tubercle is said to be; or, if this be admitted, were the circumstances and arrangement of such hydatids similar to the pulmonary vesicles? We know that hydatids are found in the peritoneum, on the tunic, and in the substance of the liver, and similar situations; but, in most of the cases which we have known, read or seen, these bodies

had attained a much larger size, and when these were minute ones, they were generally, nay, almost always, enclosed within the larger. Let us not, however, be misunderstood; we do not here say that such hydatids may not be found also in the pleura, and in the pulmonic substance; but we must doubt whether the miliary vesicles, or the nascent tubercles, are actually to be referred to this order of bodies.

3. "The matter thickens;"—we are embarrassed with more serious difficulties. Suppose we admit that the nascent or transparent tubercle is actually a hydatid, does it retain the same properties, and observe the same progress, as that body does in other parts of the system? Does it attain the same size which hydatids of the peritoneum are known to present, and do the contents continue pellucid, or at least non-purulent, as these are observed to do? Both of these circumstances are contradicted in the statement given by Dr. Baron, and must therefore be received as arguments against the correctness of his opinions. The pulmonary tubercle has scarcely been generated when it begins to become opaque in the centre, and continues, as it enlarges, to lose its transparency, until it is converted into a substance quite opaque, and entirely different from the peritoneal hydatid. Its contents have scarcely been effused in their fluid and transparent form, when they become thick and consistent, and finally end in solidity. While, therefore, we admit that the original form of the pulmonary tubercle, is a minute, transparent, or semitransparent body, with contents probably fluid, and which may be called a vesicle, we do not think that it follows, or that Dr. Baron has succeeded in proving that this body is a hydatid; much less do we think, that this hypothesis of the vesicle being a hydatid, will explain the ulterior progress, which the pulmonary tubercle undergoes.

4. Does the fluid of the ordinary hydatid, occurring in its usual situation, ever undergo the change which Dr. Baron has informed us takes place in the fluid of the minute or incipient pulmonary tubercle? Are there any well authenticated cases of hydatids occurring in the peritoneum, the fluid of which, originally pellucid, began to lose this property, as Dr. B. describes the fluid of the pulmonary tubercle, and continued to increase in opacity and density, until it was converted either into an encysted abscess, or a caseous tumour enclosed in a capsule? We must avow our ignorance of such cases, and acknowledge that we should feel a degree of scepticism concerning their occurrence. Any hydatids which we have seen or known, contained a limpid watery fluid, or at least a fluid not in any degree similar either to purulent, much less to tuberculous matter; and we never remember to have known any thing like a hydatid, containing dense caseous matter. The mere circum-

stance, indeed, implies a contradiction in terms; for no pathologist would think of applying the term *hydatid* to such a tumour. We are aware that instances have occurred of the expulsion of hydatids from various purulent or sero-purulent tumours; but we have always been taught to believe, that the formation of the purulent fluid in this case, was either accidental, or the effect of the irritation created by the hydatids, and not the result of any process going on in the interior of these bodies.

Lastly. Our author's idea of the formation of pulmonary *vomica*, is, though quite consistent with his pathological principles, the most extraordinary of all his conclusions. We have tried every expedient to render it familiar to our minds, and have hitherto been unsuccessful in all. We do not however deny, that the notion is ingenious; and we have even gone so far as to think that it may occur in some few cases; but, still, all our early principles,—all that we have learnt, that we have seen, that we could suppose,—is completely at variance with this inverted procedure in the formation of the pulmonary vomica. We have seen examples of this disease, in which we were led to believe that it had been the direct result of inflammation;—the conversion, in short, of a portion of lung into purulent matter; examples in which we could have thought it the result of secretion from a peculiar cyst; examples, in short, in which we could have thought it the conversion of one or more solid tumours into purulent matter. But, that it was the effect of a minute vesicle, gradually increasing in size, and, as it increased, converting its transparent contents into an opaque fluid, is an idea which it never occurred to us to entertain. It is needless to say that M. Laennec's dissections and observations caused the first of these ideas to give place to the third, which is now rendered untenable by the last,—adduced by Dr. Baron. We acknowledge, that we have at present no means of refuting the opinion of our author; but it is equally certain, that we cannot discover any proofs or evidence of its truth. It must, indeed, be classed among those ambiguous doctrines which may be true or false; and which, being situate in a doubtful point on the confines between truth and error, may be claimed indiscriminately by the partisans of either side.

These observations, which we trust we have placed in as clear and intelligible order as possible, will probably enable our readers to estimate the strength of the evidence, and the conclusiveness of the reasons, with which Dr. Baron has supported his views of the pathology of the pulmonary tubercle. We have divested them, as much as could be, of all scholastic sophisms, and all technical arrangement or expressions; and we are in hopes that they will be easily understood by every one who

makes his own judgment, and the ordinary principles and facts of medicine, his principal means of investigating the merit of any new doctrine. In expressing ourselves thus, we disclaim entirely the principle of opposing a doctrine because it is new and at variance with what we have been taught or early imbibed; and all who know us will admit, that there is nothing which we so much dislike, and of which we so completely disapprove, as this mode of fixing bounds to all knowledge, or opposing barriers to all investigation. Trained as we have been, not merely to the knowledge of what are esteemed the correct pathological principles of the day, but also to the observation of disease in the living body, and the examination of its causes or effects in the dead, we confess that we have been accustomed to consider the principles which Dr. Baron assails, as the most consistent with what nature presents, and founded on phenomena, though not more uniform, yet more general in character than any other on which a system could be reared. While therefore, we do not profess to be so attached to any system as not to relinquish it on evidence of a general and conclusive nature, we cannot quietly see the whole established order of phenomena in any pathological process inverted or overturned, without requiring sufficient reason for such an innovation. Let Dr. Baron collect and adduce cases which demonstrate unequivocally the series of pathological changes, for which he has so strenuously contended;—cases in which the vesicle, with transparent and fluid contents, may be satisfactorily shown to be converted into globular bodies, containing opaque, semifluid, or even solid contents; and cases in which one or more of these bodies, examined at the very period when their contents are still fluid but opaque, truly constitute the puriform deposites named *vomicæ*;—and let these cases be so numerous that they shall not, by the candid inquirer, be deemed exceptions to, or deviations from, the ordinary course and character of events in the progress of tuberculous disorganization. Let Dr. Baron perform this inquiry in such a manner as not to interfere directly with the opinions or observations of cotemporary pathologists; and we cannot doubt that the merit of his principles will gradually ensure their general adoption,—truth will prevail over error,—and a mere hypothesis, suited only to a few isolated facts, be converted into a legitimate theory, established on a solid and permanent foundation.

We ought to conclude here; but justice to Dr. Baron prevents us from closing the volume, without adverting to the treatment which he recommends for tuberculous diseases. It affords us great pleasure to remark, that, whatever be our author's pathological principles, they do not exercise on his practice any influence of which the most judicious physician would not ap-

prove. He is led, of course, to dwell much on the means of promoting absorption; and, to accomplish this desirable object, he recommends exactly the same course, which has been long taught and practised in the treatment of consumption. The only novelty which we can recognise, is an extreme confidence in the powers of iodine, and its alkaline preparation, the hydro-iodate of potass. The efficacy of this medicine in controlling various organic changes, and the hopeless nature of tuberculous diseases in general, are sufficient to encourage the trial of this remedy. We have had occasion, with regret, to add it to the list of unavailing means. We trust Dr. Baron's exertions will be rewarded with more gratifying results.

A Treatise on the Epidemic Puerperal Fever, as it prevailed in Edinburgh in 1821 *and* 1822. By W. CAMPBELL, M.D., *Lecturer on Midwifery, &c.* Edinburgh, 1822. pp. 303. 8vo.

A Treatise on the Disease termed Puerperal Fever, illustrated by numerous Cases and Dissections. By J. MACKINTOSH, M.D. Edinburgh, 1822.

(From the Quarterly Journal of Foreign Medicine and Surgery.)

Before the appearance of these works, reports had already reached us of the existence of epidemic puerperal fever in Edinburgh, but we were far from being aware of the extent to which it had proceeded, or the fatality by which it had been attended. Another circumstance is also but too evident, namely, that the different views entertained by different individuals on the nature and treatment of the disease, have given rise to some unfortunate dissentions, but too much resembling those which have so often been witnessed in that city.

We may mention among others, a pamphlet entitled "Notes on Dr. Mackintosh's Treatise on Puerperal Fever, by James Moir, Surgeon," which reminds us of another of a rather older date, bearing the *assumed* name of "James Johnson." It is a singular mark of a bad cause, or at least of cowardice, when a controversialist thus shrinks back from the brunt of the battle and screens himself behind a man of straw. It is most evident that Mr. Moir, if not Dr. Hamilton's amanuensis, must have been very familiar with his MSS., and at all events authorized to copy from them *ad libitum*. We certainly do not admire his genius for selecting, nor his scurrilous vituperations of the authors under review.

Of the two books which head the article, we give a decided preference to that of Dr. Campbell. He has judiciously con-

fined himself to the task of relating what (and in what light) he observed in the disease, and the treatment that he employed with success. Dr. Mackintosh, on the contrary, has undertaken to treat on the disease at large and to illustrate its pathology. We fear he has been too hasty, and not altogether aware of the extent and difficulty of his subject. For our own part we cannot see that he has added any thing of consequence to what has been said by Gordon, Hey, and particularly Armstrong. At the same time we do not fear the charge of contradiction or inconsistency in saying, that he is not without merit, and that there are portions at least of his work both novel and ingenious, and which if given with more humble pretensions might have deserved and received a higher praise.

Descriptions of a disease having much correspondence with puerperal fever are to be found in the works of the early physicians, from the time of Hippocrates downwards, but until a comparatively recent period it did not attract much attention from the profession. In 1716, we are told that Strother gave it the name of puerperal fever, since so celebrated, but it was not until the middle of the last century, at which time it raged epidemically in the hospitals of London and Paris, that we had any satisfactory accounts of the symptoms during life, or the appearances on dissection after death. Until a period even yet more recent it was considered as not merely a dangerous, but as a uniformly fatal disease. The eminent practitioners of Midwifery who flourished in this country in the middle and latter parts of the past century, (and never was the science more arduously, or more successfully cultivated,) were unanimous, or nearly so, in this opinion. We may refer to the treatises of Drs. Campbell and Mackintosh, for proofs of the fatality of the disease in the Hospitals of London, Paris, Dublin, Edinburgh, and Lyons—in fact, in every situation in which it made its appearance. It must be a matter of congratulation to every feeling mind to consider, that at present, to use the language of Dr. Campbell, puerperal fever, if detected early, and treated upon sound principles, admits of being cured with as much certainty as other diseases which were at one time considered irremediable. This change has been gradually effected. Astruc, so well known by his history of the venereal disease, had, it is true, the merit of anticipating all who had succeeded him, but his instructions were unknown or neglected, and we have arrived, through the inadequate and vacillating treatment of Leake, Hulme, and Denman, in this country at least, at the more happy results to be found in the writings of Gordon, Hey, Armstrong, and in the books before us.

The first case in Dr. Campbell's practice occurred in the end of March 1821, when the weather was changeable, with sudden

variations of temperature. From that period until September 1822, of 789 patients, 79 were attacked with more or less violence, and 22 died. In this time also he understands it to have prevailed in the Lying-in Hospital, and to have proved fatal to several, although the number could not be ascertained. He supposes the disease to have occurred earlier in the city than in the Hospital.

In a great majority of cases, it appeared soon after parturition, generally within the third day. In some cases it showed itself in less than twenty-four hours; more rarely as late as the fourth to the seventh day. In all but three cases, it was preceded by one or more rigors, differing in severity and duration. The shivering is soon followed by distressing pain in the forehead and eye-balls. To the cold stage succeeds, for the most part, a hot, parched skin, sooner or later followed by profuse sweating, chiefly confined to the trunk, and rapidly lowering the increased temperature. The next symptom that attracts attention is pain in the abdomen; in general it follows the symptoms already described; at other times there was scarcely any interval between it and the afterpains, the one running into the other. This pain is described as a general soreness, convertible into acute pain by pressure. In some cases the uneasiness is so trifling, that without this essential precaution it might be overlooked.

At the commencement, there is seldom any remission of pain, but as the disease proceeds to a fatal termination, intervals of ease are occasionally remarked, giving a deceptive idea of amendment. The remissions are of short duration, the pain returning with increased severity, and the abdomen becoming so sensible, as not to bear the weight of even the bedclothes. The situation of the pain has been variously described as existing in one or both iliac regions, or in the umbilical, hypogastric or epigastric regions. In Dr. Campbell's practice it was always confined in the beginning to the hypogastrium and ilia, and he is of opinion that those who have described it as being seated chiefly in the epigastrium, have drawn this conclusion from confounding its different stages together. The abdomen is generally more or less tumid; the swelling increasing as the disease advances. The uterus can be felt above the pubes; and is extremely sensible to the touch and apparently increased in bulk.

As to the circulation, there is from the first, great derangement of the vascular system. In some cases every thing goes on well for a day or two, when the pulse increases in frequency without any other signs of disease than headache. In other instances it was quickened from the time of delivery, and did not diminish as usually happens. Dr. C. rarely found the pulse under 110 after the disorder was fairly established; on the con-

trary, it was seldom less than 120. When the disease is fully formed it ranges from 120 to 130, and after it has continued some time more frequently exceeds than falls short of 140. At the commencement, the pulsation is sometimes full, more generally hard; as the disease proceeds, becomes contracted and thready, frequently intermits, and towards the close is so weak as to be scarcely perceptible.

The appearance of the countenance is remarkable; the face is occasionally flushed; the cheeks have a deep crimson appearance, at other times they are lived, or the patient seems exhausted; in severe cases, the eye is frequently suffused with tears, and the pupil dilated. The patient lies on her back, unable to turn to either side, and presents an anxious appearance, the eyes are dull and inanimate, and she is listless and indifferent to surrounding objects. The upper surface of the tongue is white and moist, except the raphe and edges, which are of a fiery redness. In the advanced stages it becomes brown and parched, and the teeth are encrusted. The thirst, though not at first great, becomes urgent, and the smell of the breath sour and offensive. The skin at first parched, is bathed in partial sweats, and before dissolution is covered with cold clammy perspiration.

Respiration is sympathetically affected, being very cautiously performed and solely by the muscles of the chest, from the dread of aggravating the pain in the abdomen. There is often a troublesome cough. In one case bloody serum was found in the thorax after death.

At the commencement there is nausea, rarely vomiting. As the disease proceeds, vomiting occurs, first of phlegm or frothy mucus, and afterwards of dark coloured matter like coffee grounds. The bowels are generally constipated at first, though obstinate diarrhœa occurs in the last stages. The evacuations are sometimes dark brown, at others greyish or ashy, and generally frothy; always of a most intolerable fœtor. The disease frequently occurs before there is any secretion of milk, but even after its appearance it is diminished or suppressed.

The condition of the LOCHIAL DISCHARGE has given rise to some diversity of opinion, and to a fallacious attempt at diagnosis. The best observers, Drs. W. Hunter, Hulme, Leake, Gordon, Armstrong, and Mr. Hey, have given information on this point, according in every important particular. They assert that it presents various appearances, that sometimes it flows freely, at others is more or less suppressed or changed in its characters. We think that the weight of all the evidence tends to prove, that, in the majority of cases, it is rather diminished in quantity than otherwise, though the exceptions must be allowed to be many. Mr. John Burns, on the contrary, and Dr. Hamilton of Edinburgh, considers that the pathognomonic symptoms of

puerperal fever is an unaltered flow of the lochia, and that the cases in which this discharge is diminished or suppressed should be considered as of a totally different nature. This idea would be of little consequence, and might safely be passed without notice, were it not that it leads to a most fatal line of practice, and that on the part of Dr. Hamilton, at least, it has been supported and promulgated without a due consideration of the abilities and merits of opponents, who, to speak with discretion, will not suffer by comparison with him. We feel almost inclined to suspect, that Dr. Campbell has been induced, by the influence of his name, to attempt to mould and modify the conclusions he drew from his own experience into an accordance with the dicta of the Professor. This may perhaps be excusable at Edinburgh, but the case is different on this side of the Tweed; the authority here receives no more weight than its merits, and we should have felt better pleased if Dr. Campbell had set down with less hesitation what he saw and what he thought.

The fallacy of the supposed distinction is so generally admitted amongst us, that it is scarcely necessary to say any thing on the subject. The disease described by Leake, Hulme, Denman, Gordon, Armstrong, Hey, Campbell, and Mackintosh, accords precisely in symptoms with the supposititious puerperal fever of Mr. Burns and Dr. Hamilton, the state of the lochial discharge, in some of the cases only excepted. The appearances on dissection are precisely the same in both, namely, the strongest evidences of active previous inflammation. Dr. Hamilton recommends Hulme's cases as instances of his genuine puerperal fever, and yet that author says "the lochia are diminished in quantity;—it is true that at the commencement there is more or less of a suppression of the lochia;—a fresh flow of the lochia is an eligible sign."—(HULME, *pp.* 10, 13, 33.) If either disease be treated in the manner recommended by Dr. Hamilton it is uniformly fatal, or nearly so; on the contrary, if it be treated in the manner to be presently described, whether the lochia flow or are suppressed, the results are successful in a degree that could not otherwise be anticipated. The assumed ground of distinction presents no analogy to those which are adopted in parallel instances. Puerperal Fever must be considered either as a disorder of the whole system, producing local inflammation as one of its consequences, which is the opinion of Burns and Hamilton, or as a symptomatic or secondary fever, the result of an active and extensive inflammation of the pelvis and abdominal viscera in the female, more particularly of their peritoneal surfaces—the opinion of Gordon, Armstrong, and most practitioners in this country. On the Continent we may quote, as entertaining the same views, Walther of Berlin; Dubois and Chaussier, at the Hospice de la Maternité; Capu-

ron, Gardieu, and Maygrier, teachers of Midwifery at Paris; Tommassini, and all the followers of contra-stimulant principles in Italy. The doctrines so ably supported by that school, and by Broussais at Paris, render the existence of truly idiopathic fevers at least questionable; but admitting the fact, when was the state of any particular discharge or secretion assumed as the basis of a nosological or diagnostic arrangement? Do we not witness typhus and other fevers attended, during life, by the symptoms of local inflammation in various organs, and exhibiting corresponding changes of structure after death? Do we not see such fevers with cephalic, thoracic, and abdominal inflammation; and do we not find in them every variety in the existence and appearance of the secretions of the parts affected? And who has ever ventured to suppose that such varieties formed any fair cause of separation from cases otherwise similar, or required any essential differences in the mode of treatment? With such a statement of facts we leave the question—satisfied that an unprejudiced examination only is required to carry a conviction of their truth.

Our English readers may perhaps think that we have taken unnecessary trouble in proving what few doubted; and, indeed, we should have been content to have noticed this point more briefly, had one or two individuals only been concerned—a Burns or a Hamilton. But when it is considered that these gentlemen are public teachers at Universities, which had, or have considerable reputation, and that one of them is author of one of the best manuals that this or any other country can boast of, the case assumes a contrary appearance. Their situation puts it in their power to fill the minds of their hearers, (often incapable, from inexperience, of appreciating the merits of the dicta of a teacher,) with fallacious distinctions and prejudices only to be removed by the consequences of a practice, painful to their own feelings, and uniformly fatal to those doomed to fall into their hands. To use the sensible language of Dr. Mackintosh, "if each of the young gentlemen who leave Edinburgh, must wade through blood to a conviction of his errors, it cannot be unimportant to inquire into the correctness of the pathology on which their practice is founded."

Besides, Dr. Hamilton has attempted to throw an imputation of ignorance or inattention on others, which, were controversy our object, might with more justice be retorted on himself.

To return to the remaining symptoms; the urinary organs participate, though its no great extent, in the general disorder. The secretion of urine is diminished, and the patient complains of pain on attempting to void it. Before death it commonly escapes without consciousness.

The appearance of the blood drawn is generally sizy, the co-

agulum becoming smaller at every succeeding detraction. In Dr. Campbell's practice, mania succeeded blood-letting in four cases, which all eventually recovered. We do not see the propriety of connecting this occurrence with the abstraction of blood, it is more probably a conversion of disease.

The period of the FATAL TERMINATION varied; in some cases no reaction followed the rigors, and the patient sunk in 24 hours or even sooner. We do not think that Dr. Campbell has dwelt sufficiently on this, the congestive form of the disease as it is called by Dr. Armstrong, who has so well described it, and shown its analogy with similar forms of typhus, &c. On the contrary, Dr. Mackintosh appears to be fully aware of its importance, and the most valuable part of his book consists in the illustrations he has given of it from yellow fever and other diseases. Dangerous as all the forms of puerperal fever are, this one, above all, if it be mistaken or ill-treated, is more particularly so.

A greater number of the patients of Dr. Campbell died on the fifth than on any other day. One died on the first day; three on the second; four on the fourth day; seven on the fifth day; one on the sixth day; two on the seventh day; and one on the eighth day. During the dry warm months the disease was not only less fatal, but less frequent. From July to October, 1821, six cases occurred, of which two died, one being past recovery when first seen. In the two months which followed with cold rainy weather, twenty-six cases happened, of which eight were fatal. In the warm months of 1822, the disease again became less frequent and assumed a milder character; of all the cases which appeared from the end of April to the beginning of September in that year, (at least eleven) not one died.

None of the cases were complicated with erysipelas or miliary eruptions, which some have supposed to constitute a part of the disease. He did not meet with any cases presenting from the commencement symptoms characteristic of typhus except two, in which the tongue was brown and parched from the first, but without any other typhoid symptoms than headache.

The terminations of puerperal fever are in resolution, effusion, gangrene, and abscess. The last is the most uncommon of all. By proper treatment more than three-fourths end favourably.

The NATURE of this disease has given rise to a great diversity of opinions. It has been supposed to be inflammatory, putrid, bilious, or a fever complicated with local inflammation. But it is not necessary individually to examine each of these suppositions. As Dr. Campbell remarks, every symptom and feeling from the first moment of its appearance show it to be highly inflammatory; they are rigors; quick firm pulse; local acute pain; swelling and increased heat. With this idea the appearances found

on dissection precisely agree; the vascularity of certain parts is more or less increased, and we meet with those less questionable proofs of previous inflammation, gangrene, suppuration, effusions of lymph and serum. In every one of his cases Dr. Campbell asserts that these appearances were most unequivocal. We have the concurrent testimony of Leake, Hulme, Denman, Clarke, Gordon, Armstrong, Hey, and Mackintosh to the same effect. This is the case also with most of the modern and many of the ancient continental writers. As to the hypothesis that the disease is merely typhus complicated with abdominal inflammation, Dr. Campbell, though admitting the possibility of its occasional occurrence, is disposed to believe, that in the great majority of cases, there is no just ground for the opinion. He can account for the proposition of the theory by the supposition that its advocates have witnessed the disease and described it as it appears in its advanced stages. Besides, it is well known that puerperal fever is not more prevalent during typhus epidemics than at other periods, and the mode of treatment adopted as a consequence of that idea has been equally unsuccessful with most others.

He finds it more difficult than might be expected to fix the precise SEAT of the disease. The question cannot be answered merely from the appearances on dissection; for were we to consider the part most diseased as that originally affected, we should have a different situation for the disease in every three or four cases. The uterus and its appendages, the intestines, omentum, &c. are the parts affected in different situations in different cases. The only part uniformly diseased is evidently the peritoneal surface of some of the contents of the abdomen and pelvis, which it is of no very great practical importance to discover. With this point is in some degree connected the consideration of the appellation by which the disease should be designated. It is evidently incorrect and opposed to analogy to derive the name of any disease from a series of phenomena which are only secondary, and consequent on others of more importance. We speak of phrenitis, pleuritis, &c. and not of the fever which attends them. The correct term, as implying two material points, the situation and nature of the complaint, is peritonitis, to which we would add the epithet puerperal; for, as Dr. Mackintosh says, it is impossible to separate this form of peritonitis from the situation of the female after parturition.

In treating of the CAUSES, Dr. Mackintosh, in particular, has shown much judgment and correct observation. "To understand what takes place" he says "we must recur to the changes which take place with conception and continue during pregnancy. Increased vascular action, and determination of blood to the uterine organs is among the first; menstruation ceases,

and more or less of plethora occurs, partly to supply the means of growth to the uterus, &c. and partly to effect the change of the fœtal blood supposed to occur in the placenta. The arteries and veins of the uterus and neighbouring parts are prodigiously enlarged. Together with these there is a peculiar alteration in the action of the nervous system, showing itself by an increased susceptibility to impressions of every kind." At the end of pregnancy labour ensues, temporarily increasing the action of the circulating and sentient systems. Provision is happily made to prevent the inconvenience which might arise from the increased circulation in parts, now inactive. More or less hemorrhage follows delivery, and is succeeded by the lochial discharge. In the course of a few hours the breasts become turgid, and at the end of a day or two the secretion of milk is established. After delivery the nervous system is yet more susceptible than before, and the slightest impressions produce unusual effects. "An attentive view of these phenomena," says Dr. Mackintosh, "will enable us not only to account for all the circumstances attending puerperal peritonitis, but render it surprising that it does not more frequently occur. We perceive at a glance, how certain causes, such as exposure to cold, to early sitting up, irregularity in diet, the use of stimulants, anxiety, grief, or even slight mental affections, may disturb the balance of circulation, and cause an irregular determination of blood, naturally directed to the vessels most ready to receive it."

Diagnosis.—We are not inclined to go at any great length into this part of the subject. In this disease it is of more importance to ascertain the reality of its existence than to attempt any very minute discrimination. For this purpose we have only to inquire and satisfy ourselves whether or not there be acute fixed pain, or general soreness of the abdomen, particularly the lower part, aggravated by pressure, inspiration, and motion, and attended by increased frequency of the pulse. It is important to remark that the disease, as we have seen it, may be forming during labour.

Treatment.—In relation to the mode of treatment proper to be adopted, there is one essential division, namely, that into cases in which there is, what Dr. Armstrong calls, congestion; in other words, in which no reaction takes place; and into those in which the disease is fully formed. Dr. Mackintosh has spoken most fully on the first. From his experience of its success in other diseases of a similar description, he recommends bleeding in preference to other remedies, and that the patient should, if possible, be immersed in a hot bath of at least 112° at the time of opening a vein. Where a bath cannot be obtained, the feet should be put into hot water, and the body rubbed

with powdered salt, or with mustard flour. Stimulants he thinks chiefly useful in rousing vital action, so as to permit the abstraction of blood, but he objects decidedly to the frequent exhibition of brandy, &c. to the exclusion of other remedies. If we are called too late, or cannot get blood to flow, the hot bath and frictions must be principally relied upon, and in such a case Dr. Ward would apply one hundred leeches, as the speediest way of obtaining relief, on the principle that the internal parts of the system are loaded with blood, and that our object should be to restore the balance of circulation, by promoting a natural flow to the surface of the body. In spite of every attention, however, and in no case it is more necessary, a great proportion of those who are fairly attacked by this fortunately rarer form of the disease, must be expected to perish.

In those instances, on the other hand, in which reaction takes place, and in which the Puerperal Peritonitis is fully formed, an active and attentive practitioner may reasonably look for considerable success. But in this disease, as indeed in the phlegmasiæ of all extensive and important organs, every thing depends on the detection of the inflammatory process at an early period, and before sufficient time has elapsed to admit of irreparable local or general disorder. An oversight on this point cannot be redeemed, even after a very short interval; prevention is here, indeed, preferable to cure.

If we are present during the stage of depression, it is evident that the first and most important object must be, to bring about reaction as speedily as prudence and our means permit. This is to be effected by exhibiting warm innoxious diluents, by the use of the warm bath when possible, and similar measures. Emetics are of very questionable utility; they certainly determine to the skin during their operation, but the nausea and depression which succeed very much take away from this advantage.

When the shivering and chilliness have subsided, and are succeeded by moderate warmth of the skin, it will be proper to abstract blood. In this respect, Dr. Campbell thinks, that we should be regulated by the state of circulation, but to us it seems a very inadequate guide. The best are the abdominal pain, and the duration of the disease. In general we have, after reaction, found the pulse full and developed, rather than small and contracted, though the exceptions are numerous. It is improper to attempt to fix any particular quantity of blood to be abstracted; our intention is not so much to take away blood, as by so doing, to stop the progress of the disease, and this may be done as effectually in one case by ten ounces, as in another by forty; we must, in fact, adapt our conduct to the circumstances in which we are placed. Of one thing we feel convinced, that

the first blood-letting at least, should always be to such an extent as to induce syncope; any thing short of this tends only to weaken the patient, without giving any material check to the disease. To accomplish our object, without unnecessary loss of blood, the patient should be raised to a sitting posture if circumstances permit. With this precaution we have seldom found it necessary to take more than 20 to 24 ounces, rather under than over the larger of these quantities. With the same view, the opening in the vein should be extensive, or even two might be opened at once. In the more advanced stages of the complaint, the same vigorous treatment is hardly admissible, though we cannot conceive a case in which, with a conviction of the diseased processes continuing, as evinced by pain and other symptoms, and without any signs or doubtful ones of approaching dissolution, it would not be proper to abstract a quantity of blood proportioned to the severity of the patient's sufferings, her strength and other circumstances. Setting aside this question, there cannot be a doubt of the propriety of speedily repeating blood-letting, if the first should have given imperfect, or only temporary relief, nor should we wait until the aggravation of the symptoms proved that the disease was unextinguished; we must anticipate its ravages, and check its first efforts at relapse. As Dr. Campbell observes, we know that without bleeding the patient must perish, and that with extreme suffering; whilst with it, if we fail to cure, we are, at least, able to palliate.

When we have recommended bleeding, we have said every thing that is essential to the cure of the disease; other remedies are of secondary importance. Local bleeding is a most useful auxiliary, and may be resorted to in cases, and at times that will bear venesection; but to be attended with real advantage, it should be practised boldly; not 12 or 20, but 60 or 100 leeches, followed by warm fomentations, which, indeed, are uniformly soothing and anodyne. Dr. Mackintosh speaks favourably of blisters, but we are more inclined to agree with Dr. Campbell, that they tend to increase irritation already existing, and to obscure the complaint. Dr. Pemberton, in his Work on Abdominal Diseases, objects to them in peritonitis, as they render it difficult to distinguish whether or not the belly be painful on pressure. If applied at an improper period they may be positively hurtful: we have been informed of cases in which the points of the peritoneum, precisely corresponding to the blistered surface were found, after death, more vascular and inflamed than any others. Neither do we expect much from digitalis; it seems to us by no means suited to the cure of acute inflammations, and we have seen it produce all its noxious effects,

without materially lowering the pulse, or at all checking the disease.

Purgatives certainly rank next to blood-letting, and are often sufficient, after one bleeding, to effect a cure. The natural tendency of the disease is to terminate in diarrhœa, and the best observers have remarked the temporary relief, which, even in fatal cases, attends every evacuation; in such instances it is the last effort of nature, and the attempt to stop it is attended by still more dangerous consequences. The particular purgative employed, is in some degree a matter of indifference, and our choice must be regulated by the habit and condition of the patient. We are in the habit of employing calomel and jalap in moderate doses, followed by a dilute solution of salts, as it is well known that a smaller portion of a saline purgative, considerably diluted, acts more effectually and more speedily than a strong or saturated solution of a larger one. We have never tried either turpentine or half drachm doses of calomel; as to the latter, we believe it a mistaken idea that it is to be depended on as a purgative, and any other effects from its use, are perhaps even more certainly attained by smaller doses. Drs. Mackintosh and Campbell are both opposed to the plan. They likewise agree in discarding turpentine. With Dr. Mackintosh we may say, that on principle we never saw a case in which it would have been admissible, and as a specific we feel little disposition to use it. "When it is successfully employed in peritonitis or enteritis," then, says he, "will I give it a trial in this disease," and we entirely agree with him.

We have little to say of other measures; the progress of the disease to death or recovery is so rapid, that there is little time for the employment of ordinary pharmaceutical agents: but those general principles which every practitioner is supposed to be master of, must be applied in this, as in every other individual case, with those limitations and modifications which its nature may require.

We are far from having exhausted the subject, or even given an account of every thing that deserves notice in the valuable treatises before us. With such an extensive and weighty subject, all that we could do, was to select and bring together whatever was best ascertained, most important, and most liable to be mistaken. It is a duty incumbent on every man who practises any part of medicine, but above all midwifery, to make himself familiar with the nature and treatment of one of the most dangerous, and, till lately, most fatal diseases that exists. If what we have here said, should promote the acquirement of this knowledge, or help to throw any light on these points, our purpose will be fully answered.

Memoria sullo Scirro e sul Cancro. (Transactions of the Imperial Institute of Milan, 1822.)

Remarks and Practical Results of Observations on Schirrus and Cancer. By ANTONIO SCARPA, Emeritus Professor and Director of the Faculty of Medicine in the Royal Imperial University of Pavia; Chevalier of the Royal Order of the Iron Crown; Member of the Royal Academy of Sciences at Paris; F.R.S. &c. *Translated from the Italian, with an Appendix, and some Annotations on the Text.* By JAMES BRIGGS, Surgeon to the Public Dispensary, and Assistant Surgeon to the Lock Hospital.—8vo. pp. 61. Cadell, London. 1822.

(From the London Medical and Physical Journal for April, 1823.)

The practice of surgery has its stumbling-blocks, as well as that of medicine, and cancer may still justly claim the foremost rank among these opprobria of our art. If every pathological subject had been simplified or illustrated in proportion to the pains bestowed upon it, and to the number and reputation of those authors who, from the remotest antiquity to the present time, have exerted themselves to clear away the obscurities and to solve the difficulties which yet continue to perplex the practitioner, and render the diagnosis a matter of contention,—schirrus and cancer would long since have ceased to be a reproach to the profession, or so fertile a source of emolument to the unprincipled empiric. But the subject is in itself extremely intricate; a long life and a very extended experience will hardly afford to any one man a sufficient number of opportunities of developing the formation, growth, and termination of this disease, so as to arrive at an exact knowledge of the peculiar changes of structure which form the essential characteristic of the true schirrus. Add to this, the number of important questions that present themselves in the course of the inquiry, and which reasoning cannot influence, but which must be the direct deductions of experience and observation, and that of a person whose mind is free from all preconceived theoretical views, and we shall then have some faint idea of the real difficulties of the task. LE DRAN commences his treatise on Cancer with the following remark:* "Ce n'est que dans les observations qu'on peut puiser des connoissances sur la nature du cancer, et sur la manière de le traiter;" and yet it must be confessed, that the manner of making an observation is not of trifling importance; and upon this subject we cannot forbear quoting an extract from an author of our own age and country, which puts this

* *Memoirs de l'Academie Royale de Chirurgie*, tome iii.

truth in a clear light, equally impressive from the truth of the remarks, and the elegance of the language in which they are couched.*

"While we remain unfurnished with authentic standards by which all observations may be examined, it ought not to excite any surprise if the same name be assigned to two complaints, the histories of which are repugnant to each other; or if opposite modes of treatment be directed for diseases that bear a common appellation. To this want of discriminative principles it may be ascribed, that simple facts have been often perplexed by the subtilty of specious distinctions, and that the mind has been bewildered by the minute ramifications of genera and species. It may also be imputed to the same defect, that affinities have been so frequently discovered, and resemblances traced, where nature had impressed an incompatible disparity.

"The clouded, ambiguous, undecisive state of the art of healing, hath been confessed and lamented in every age: it hath often furnished the gay with materials for wit, and infused into the more sober scepticism or disgust. Many different causes have been assigned, on various occasions, for the slow progress and imperfect condition of this important branch of natural science; but, among the several impediments that have been mentioned, I do not recollect that the almost incommunicable nature of medical knowledge hath been noticed by any writer. The term *incommunicable* must here be understood in a qualified sense,—not as implying that the science is in itself mysterious and inaccessible, but that the knowledge of the experienced practitioner must always be transmitted inadequately, and with difficulty to the mind of the student. * * * For, as language is not rich enough to furnish words that will perfectly denote all the different shades of colour, though their dissimilitude is obvious when presented to the mind, so there is a species of practical knowledge, composed of simple ideas derived from observation, for which no competent terms have yet been contrived, and which no periphrasis can adequately describe."

But, independently of these considerations, it must be conceded that many of the authors themselves, who have written upon this subject, have tended greatly to mislead us in the inquiry, and consequently to retard its progress. The intellectual, as well as the natural world, sometimes presents to us false lights, whose brilliancy attracts, but which lead only to disappointment and error. Substituting some vivid and agreeable fancy for the more sober but laborious research after truth, these men first form an hypothesis plausible and captivating, and afterwards torture all the facts of the case to support their preconceived notions. We allude more especially to Dr.

* Pearson's *Practical Observations on Cancerous Complaints*, (Preface.)

ADAMS' hydatid theory, which, although now fortunately exploded, continued for a long time, by its novelty, and even by its eccentricity to mislead the profession.

Upon the subject of cancer, indeed, we are almost afraid that the labours of the last century have thrown, in reality, no additional light: the merit of most of the best modern publications consists rather in clearing away the incumbrances and misconceptions that involve the disease,—in settling some points of practice relative to the period of operation,—in destroying the pretensions of some favourite specific remedies,—in distinguishing those affections most likely to be mistaken for this formidable malady,—and in endeavouring to solve the main question relative to its local or constitutional origin.

The pamphlet before us was originally published in the Memoirs of the Imperial Institute of Milan: its essential doctrine is that of the local origin of schirrus and cancer; and Mr. BRIGGS has conferred an obligation upon the profession, in translating and publishing the opinions of a man so justly celebrated as SCARPA, and which otherwise would have been inaccessible to the majority of the profession in this country, at least. We shall now, therefore, proceed to the analysis of the work, offering from time to time such remarks as have suggested themselves to our mind from the perusal.

In a short preface, the translator states his belief that the fact of the local origin of cancer is made out in this little work, from a "more satisfactory body of evidence than has ever been yet laid before the public." It appears also to be his conviction, that the search after a specific remedy for the cure of this disease is a vain one; and that, from the facts obtained by the anatomical investigation of the schirrous tumour, no more rational expectation can be entertained of the restitution of a part, the organical structure of which is found to be wholly subverted, any more than of the dispersion of an encysted tumour, or opake lens of the eye, by such means. The great point to determine he, therefore, justly concludes to be, the precise period when the removal of the diseased part may be effected with the fullest assurance of success. The Appendix contains an account of a disease of the breast, usually considered as cancerous, and "which has been introduced, not merely as tending to illustrate the author's opinion upon this subject, but because it seems to throw some light upon a complaint, of which, from its rare occurrence, we possess but very imperfect information, and which, left to itself, appears to have uniformly a fatal tendency." There are also some notes illustrative of the author's opinions.

The pamphlet commences with an enumeration of those points relative to schirrus and cancer, upon which the profession appear to be still divided, and which are these:—1st.

Whether schirrus be purely a local, or a general as well as local affection? 2dly. Whether it attacks, without exception, every organic texture of the human body? 3dly. Whether there are any certain marks by which it is distinguishable from every other disease which bears an affinity to it? 4th. Whether its malignancy is equal in degree in all parts of the body?" and these questions lead us directly to inquire, lastly, why cancer, especially the "legitimate glandular," so constantly baffles the hand of the operator, and the action of the most powerful caustics or the actual cautery.

In considering these questions, our author does not adhere to the order in which he has stated them, but commences with considering the second point; and he decides that schirrus and cancer never attack primarily the lymphatic system, nor of necessity the absorbent glands: the mucous glands he also considers as exempt from this affection, as well as all the viscera, with the exception of those internal parts which are furnished with an intimate covering of the reflected skin,—as the larynx, œsophagus, stomach, rectum, vagina, and the neck of the uterus; so that he does not admit the indurations of the brain, liver, &c. under the head of cancerous diseases, but as "morbid phenomena of general scrofulous diathesis, or else indurations either referable to previous acute inflammation imperfectly discussed, or produced by slow and long protracted inflammatory action." (p. 5.) In these remarks we entirely concur; and we consider it no small advance to a knowledge of the true nature of this disease to confine the term strictly to its original meaning: to call a disease schirrus, because *hardness* is its most striking symptom, is likely to prove a fertile source of mistake, and to lead to most unnecessary severity in the treatment. We are quite aware that the liver and other viscera are frequently the seat of the medullary fungus, which Mr. Hey first introduced to the especial notice of the profession, by the name of the Fungus Hæmatodes; but we are convinced that this disease is altogether different in its leading points from true cancer; that it is unphilosophical and mischievous to confound them with each other; and that they present no point of resemblance, either in their external appearance or internal structure, and are only indebted to the relationship that has been supposed to exist between them, to the equally intractable nature and fatal termination of each. We are happy in having these sentiments confirmed by the opinion of so celebrated a man as Scarpa, though several eminent authors of our own country had previously advocated the same opinion.

With regard to the mucous and salivary glands never becoming the seat of cancer, we find that, although this is the general

opinion of the best writers, still Mr. Pearson* observes, that he has seen the parotids affected with a true cancerous disease. BOYER asserts the same fact to have occurred in his practice; but, with this single exception, there seems to be no reason to doubt the exemption of the salivary glands in general.

The next point which our author inculcates is this, "that schirrus and cancer never make their appearance before puberty, and seldom before the twenty-fifth year, in either sex." Here we think the period named too early. In the breast and testes, for example, the true schirrus is seldom called into action until that period of life when their respective functions have ceased, or nearly so; for, whoever will take the pains to peruse the almost endless number of cases of tumours taken from the breasts of females under the middle period of life, will find strong reasonings for doubting, both from the history of the case and the dissection of the tumours themselves, whether they were not extirpated more in compliance with the fears of the patient, and the vague and uncertain knowledge of the surgeon, than from any real proof of their true nature. It would be invidious, after the above remark, to point out any particular cases; but many such are to be found in most of the publications on cancer, even within the last fifty years.

"Observation and experience, (says M. Scarpa,) teach us that cancer is never formed but in consequence of legitimate schirrus affecting some of the external conglomerate glands, or of rigid porri, or warts, or malignant tubercles of the external or reflected skin, partaking of the nature of schirrus." Among the glands, the mammary is the most frequently subject to the disease; then the parotid, maxillary, and lachrymal glands, and the body of the testicle; for he doubts whether the epididymis is ever the primary seat of schirrus. In the skin, the schirrus presents the appearance either of a hard wart or tubercle, or dark varicose knot; yet, though these three external characters, are different, the internal structure of them all bears a close resemblance to the texture of the glandular schirrus, is intersected by whitish lines, and filled with a viscid albuminous fluid.

With regard to the cancer of the uterus, our author speaks confidently, from the result of anatomical and practical facts, that the primordia of cancer of the uterus are to be traced to the ulceration of one or more of these small schirri, which are formed upon the reflection of skin investing the upper part of the vagina, and the orifice and neck of the uterus. Neither is there, he continues, in the whole records of surgery, any well-authenticated fact of cancer of the uterus having originated from any other part of this viscus.

* *Practical Observations on Cancerous Complaints.*

We come now to the most difficult part of our subject, the *diagnosis of cancer;* because it is quite evident that, for all practical purposes, the most efficient of these marks which our author mentions,—namely, the internal texture of these tumours, cannot be any guide in forming our opinion as to the nature of any particular case. Here the attentive consideration of the preceding phenomena of the disease, and all collateral circumstances, can alone enable us to form a correct judgment: it is not one symptom,—it is the whole taken together, and in connexion with each other, that can alone guide us to the truth. M. Scarpa thinks there can be no difficulty in distinguishing schirrus from all encysted tumours, and that by the stony hardness of this latter disease alone. From scrofulous enlargements we must learn to distinguish it by the habit of body, and the known symptoms of scrofulous predisposition: in adults, cicatrices upon the lymphatic glands, which had suppurated in infancy, will sometimes assist us. "The scrofulous taint of the habit is likewise never confined to one of the external conglomerate glands alone; but affects, at the same time, a greater or less number of lymphatic glands in various parts of the body, particularly in the neck, groins, and axillæ." (p. 12.) The smoothness of the tumour, its comparative softness, are also readily distinguishable by the hand of the surgeon; and, lastly, the dull sense of pain which accompanies this swelling, and which gradually increases as it advances in growth. Our author observes, that, in the testes, scrofulous enlargement is *never* isolated, but is always associated with a similar affection of the lumbar and mesenteric glands. This distinction, however apparent in the dead body, it must be evident cannot alone be a sure guide during life, and consequently will not always avert the recommendation of an operation: for our author himself says, that neither the diseased lumbar or mesenteric glands admit of being felt upon the living subject; and it is only after the excision of the testicle that the increased volume of these glands, and the marasmus consequent thereupon, too clearly prove that the nature of the disease had been mistaken. M. Scarpa is careful to distinguish this condition of the testicle from the malignant soft fungus which sometimes attacks that gland. In this latter case the lumbar glands enlarge, in consequence of the irritation of disease: in the scrofulous affection, their swelling is of a common and simultaneous origin. In these cases, our author asks whether tying the spermatic artery in the groin simply, in the manner in which it has been performed by Professor MAUNOIR for the cure of sarcocele, may not be sufficient, both to arrest the disease of the testicle, as well as the enlargement of the lumbar glands?

We now come to an enumeration of all those marks which,

when met with together, leave no doubt as to the nature of the affection. The schirrus is a disease of advanced, or at least of middle age; it attacks those of a sanguineo-bilious temperament, in whom the general vice of the habit bears no suspicion of scrofula. It is a solitary affection. It is excessively hard and indolent; it increases slowly in every direction; its insensibility is preserved, notwithstanding its increase in size, until it degenerates into cancer. Here we must beg leave to dissent: the presence of occasionally lancinating pains, or (*eclairs de douleur*, as they are called by the French,) frequently denote the commencing activity of this affection; and, although the tumour is insensible to the touch, these darting pains will, at intervals, cause much suffering to the patient. Still it is true that the genuine schirrus is not accompanied by constant pain; which, indeed, appears to be inconsistent with its character. It does not uniformly increase in size; for, in the tuberculated schirrus, as soon as the darting pains commence, so far from increasing in size, it shrinks, drawing inward along with it that portion of the skin to which it has formed adhesion.

The above characters sufficiently distinguish the disease from struma,—from tumours of the conglomerate glands, caused by acute inflammation, which has either been neglected or repelled, —as well as from the medullary fungus, which has its origin in the subcutaneous or intermuscular texture; the remarkable elasticity of these tumours is another characteristic.

The above criteria apply to the diagnostic of glandular schirrus; the following observations relate to those warts, or malignant tubercles, of the external reflected skin. "The real nature of these (he says,) is to be judged of from their universal rigidity and hardness; from their being observed to be deprived of their natural integument, which, when they are benign, retains them within certain limits upon the skin; from the unusual size and depth of the base, which seems to extend beyond the substance of the skin; from their being of a yellowish, livid, or dark colour, surrounded by a red circle; from their rapid and almost sudden growth; from the intolerable pruritus excited by them; from the fissures that are formed in them, from which there is a slight discharge, at times, of yellowish, sanguinolent, acrid serum, preceded by sudden darting pains." (p. 21.)

It is a great error in practice, says M. Scarpa, to suppose that any hard, chronic, or indolent tumour may, from lapse of time or other circumstances, be changed into cancer. It is certain that any such tumour may degenerate into a sordid, fungous, spreading ulcer; and of which he adduces many instances. Twice our author had an opportunity of examining a supposed open cancer in the breast, which had existed some years.

The patient had complained of an uneasy smarting sensation, but never of darting pains; the ichor discharged from them had not the *lixivial odour;* and the general habit of body was such as is usually met with in scrofulous subjects, and had been so from infancy. Such cases have been noticed by Mr. ABERNETHY, Sir E. HOME, Mr. BELL, and indeed by all the writers on this subject: but the disagreement upon this point is not, practically, of much importance, since it is only in the early stages of the disease that the art of surgery can afford any prospect of relief in true schirrous affection.

We now proceed to give some account of our author's observations upon the *structure of the diseased parts*, and which we have postponed until we had collected together all his remarks relative to the diagnosis of the disease.

"In regard to the difference between the internal altered texture of the strumous conglomerate gland and that of schirrus, the following are the results furnished by anatomical investigation. A coloured size-injection, thrown into the arterial vessels of the strumous gland, penetrates it at first very freely, but becomes soon effused into it, in consequence of the flaccidity and friability of the vessels which supply the substance of the gland. When a section is made of the strumous gland, a vascular compact substance presents itself, filled with a glutinous (*albuminous*) fluid, mixed with a granulous, sebaceous, or cretaceous substance. Between the body of the tumour and its exterior involucrum, there are always some vestiges of *coagulable* lymph; and frequently also, in the internal part of it, indubitable proof of its having suffered inflammation, though slight and passive. In the schirrous tumour, on the contrary, the injection, though of the finest kind, never fills but the principal arterial trunks of the diseased gland. The hardness of the substance composing the schirrus is altogether peculiar to this tumour, and cannot be confounded, even by the most inattentive observer, with that of any other kind of chronic glandular enlargement. It has the appearance of softened cartilage, and strongly resembles the mollified substance of the cartilages and ligaments of the joints, in the change observed in the *white* swelling. When the schirrus is divided through the centre, it presents a whitish surface, equally striped with bands still whiter than itself, like rays drawn from the centre to the circumference, or in the form of ramifications. On pressing upon it, a transparent gelatinous (albuminous) fluid issues from it, which, diffusing itself upon the cut surface, renders it in a short time lucid, and as if it were covered by varnish. Lastly, if the struma and schirrus are macerated together in water for a considerable length of time, the struma is found to separate into a soft, spongy, fimbriated mass; while the schirrus retains nearly the peculiar hardness which it possessed before it was subjected to that process. This remarkable difference in the cohesion between the component parts of the schirrus and those of struma, points out the reason why, in some supposed cases of oc-

cult cancer, (but which in reality were of a strumous nature,) cavities have been found containing quantities of pure or bloody serum, of two, four, and even six pounds weight, which certainly could never take place in the intimate, hard, tenacious substance of schirrus."

Our author considers it probable that the gelatinous fluid condensed in any of the external conglomerate glands, as well as the *liquamen* which is fixed and coagulated in the intimate texture of warts or tubercles, is as it were a matrix, containing the germ of a peculiar morbific poison, in a latent or inert state, and which only requires the concurrence of particular circumstances to develop its noxious qualities. Whatever obscurity, he goes on to say, there may be with regard to the primary origin of the morbid principles generated in the animal economy, still he argues, from the example of erysipelas, farunculi, the exanthemata, and numerous others, (many of them, in our mind, however, of a very questionable nature,) that germs of disease are actually formed in the general system, which, not being miscible with the blood, are afterwards, by the power of vitality, expelled entirely from it, or driven only to the skin, or else deposited in some of the external emunctories, and there held, for a greater or less time, in a latent and innocuous state. (p. 26.) In conformity with these views, our author inclines to the belief that the schirrus is only the deposition of the seed of a more formidable malady than schirrus itself, and which remains quiescent until, by a concurrence of internal or external causes, it is brought into activity.

Our author is inclined to doubt the existence of a schirrous *diathesis:* 1st, because it is an isolated disease; 2dly, because, if extirpated before it begins to degenerate into cancer, it is radically cured. With regard to predisposition, he observes:

"Another circumstance to be estimated in investigating the primary origin and essence of schirrus, is that of the *predisposition* to this sad affliction. For, of a number of persons of both sexes exposed to the same combination of causes, which in the schools are called excitant (*produtrici*) of schirrus and cancer, the whole are not attacked with this disorder. I see but in all these causes only such as are *occasional;* as, for example, the suppression of the menstrual flux in women, the hæmorrhoidal in men; the disappearance of cutaneous acrimonies, or of the rheumatism, external violence, deep and long-continued grief of mind, the abuse of venery, and the like. And it is right to observe, that these are not even the only occasional causes; for there are numerous examples of women affected by schirrus and cancer, where the menstruation is regular, and of persons in whom it is neither to be attributed to the suppression of habitual sanguineous evacuations, the disappearance of cutaneous acrimonies, of rheumatism, irregularity in living, or

long-continued mental distress. The *efficient* cause, then, of this disease is to be traced to no other source than that of internal elaboration, to which every individual is more or less, or in no measure, predisposed, although exposed to the same *occasional* causes. For these reasons, I do not believe it to have been in any manner proved that there are schirri and cancers which are solely produced by external *causes*, to which some writers have been pleased to attribute a character less formidable than that which is met with in the same disease proceeding from internal morbid action: and my assertion is also founded on experience."

The symptoms which announce the development of the *latent* disease, and the commencement of the second stage, are a pruritus and a sense of burning heat within the substance of the schirrous gland, without any alteration in the appearance of the skin that covers it; to these succeed darting pains, which are not increased by making pressure on the tumour. If the gland which has arrived at this stage of the disease be cut into, instead of the hard uniform substance striped with whitish bands, the appearance is that of a hard lardaceous mass, dotted here and there with red points, excavated in many places by cells of various sizes and depths, containing a viscid, cineritious, sanguinolent fluid, of a highly acrid quality.

Our limits do not permit us to follow our experienced author in his vivid and accurate description of the progress and termination of this appalling disease; a description which applies to the most formidable and regular progress of the cancerous ulcer. It must be recollected, however, that sores, the cancerous nature of which cannot be doubted, do not invariably follow this course, nor assume exactly the appearances here described; and the cases to be found in all modern writers completely prove this fact.

Mr. Pearson has quoted some cases from Wiseman, which, at the period when he published his observations on Cancer, he doubted whether he ought to consider as fair specimens of that disease, but which subsequent experience has induced him to look upon as real instances of cancerous ulceration. The translator of the work before us was authorized by Mr. Pearson to state this alteration in his opinion.

We have now arrived at the practical application of our author's principles; and it ensues, as a necessary consequence of the premises, that the demolition of the schirrus can never be attended with success, unless the operation shall have been executed previously to the development of the latent morbid seed in the substance of the schirrous gland, or malignant wart or tubercle of the skin; that is to say, before the commencement of the lancinating pains, and infection of the lymphatic glands connected with the seat of the occult cancer: and the instances

of success which are to be found in RICHTER, MORGAGNI, and others, he justly concludes to have been diseases of a totally different nature from the real cancer; and it is upon these grounds that he is disposed to reconcile the very discordant results of the operations recorded by HILL, BENJAMIN BELL, by the elder MUNRO, CALLISEN, BOYER, &c. In the fatal cases the operation was performed too late,—that is, after the morbid leaven had become developed; and in the first-named instances many other diseases were included in, and contributed to swell the number of fortunate results.

Mr. Pearson, in delivering his opinion upon this practical point, (i. e. upon the proper period of operating,) supposes, says our author, that the schirrus is never, even from its first appearance, confined and circumscribed within the limits of the gland which it occupies, but that the disease extends beyond the sphere of it, and is constantly associated with other smaller schirri, which, from their extreme minuteness, escape observation. By waiting until the principal schirrus has acquired its greatest augmentation, an advantage is obtained of being able to extirpate, together with it, all the surrounding small schirri, which are now become perceptible to the eye and touch of the operator. It will be seen, by referring to Mr. Pearson's work, (p. 31.) that M. Scarpa has in this instance mistaken that gentleman's meaning,—a point which has not escaped the penetration of the translator; and we must therefore, in justice to our eminent countryman, quote the passage in question, in order to show how conformable with reason and truth his opinion is, and how extremely different from the opinion erroneously attributed to him in the pamphlet before us.

"I believe, (says Mr. Pearson,) it is generally true that those morbiferous poisons which have a tendency to perpetuate themselves, taint the constitution more deeply in proportion to the length of time they have acted upon it. But the comparative security from, or danger of, a relapse, can by no means be estimated from the duration of a cancerous complaint. The early extirpation of a cancer confers no peculiar security against the return of the complaint: on the contrary, if the removal of the part were equally complete in two patients, one of whom had been afflicted seven months, and the other seven years, with a cancer, I should esteem the latter patient in less danger of a relapse than the former. My reason for an opinion which to some people may appear singular, is this:—that, when the breast, for example, is affected with cancer, distant parts of the gland may become the seat of the morbid alteration about the same period. These several diseased portions may not advance with an equal celerity; but, while one portion has acquired a considerable bulk, the other altered parts may be scarcely objects of attention. Under such circumstances, the more obviously morbid

parts may be removed; but, the disease being only in progression, no man can be certain, without *removing the whole breast*, that he has not left some diseased fibres.* If, however, the disease shall continue without increasing during several years, one may, in general, conclude that its boundaries are more accurately defined."

It is scarcely necessary to say, that this doctrine is in conformity with the sentiments of SHARP, LE DRAN, and all the best authorities, not excepting that of SCARPA himself.

We shall pass over the quotations from the works of HIPPOCRATES, CELSUS, LUD. MERCATUS, and SEVERINUS, who are all introduced by our author to prove his main point, that the operation can only be undertaken with a prospect of success in the first stage of schirrus; for he employs the term *occult cancer* to denote the second stage, or period when the schirrus has become active. This brings us down to the present age; when, after extracting the opinion of Le Dran,† which tends to the same point of practice, he presents us with the result of the experience of a countryman of his own, FLAJANI, who has performed the operation of extirpating a considerable number of *genuine schirri*, within the first months after the appearance of the disease. Of twenty-seven operations, two patients only have experienced a relapse; the remainder have been perfectly and radically cured. Our author confirms this testimony by his own experience. In the whole course of a long practice, he declares that he has only succeeded in the extirpation of three cases of *genuine* schirrus; these being the only three in which he was so fortunate as to perform the operation within the first months from the appearance of the disease, before the unpleasant sense of pruritus and heat had taken place, and, consequently, that of the lancinating pains. The operation, in all instances, was performed by the removal of the whole mammary gland. In the legitimate schirrus of the testicle, he has, however, effected a cure in a much larger proportion of cases; a difference which he attributes to the more frequent opportunity of extirpating the schirrous testicle during the first stage, than that of amputation of the schirrus of the breast, before it has advanced to the state of occult cancer. (p. 47.)

With regard to warts and malignant tubercles of the skin, he is inclined to allow more liberty with respect to the period of operation; it appearing to him, as it did to Le Dran, that these are of a less virulent nature than the glandular schirrus, or even the malignant tubercles of the reflected skin. Experience has, with our author, confirmed the truth of this remark; having

* See some cases of Cancer, by Mr. C. Bell: *Medico-Chir. Trans.* vol. xii.
† *Mem. de l'Acad. de Chirurg.* vol. iii.

been successful in some cases of this kind, in which not only the darting pains had commenced, but where even fissures were formed, from which there was an occasional discharge of corrosive serum. He, of course, unites the wound, and heals by the first intention.

The pamphlet concludes with the case of a Tyrolese gentleman, aged seventy-four, who had suffered for four years from the presence of three warts upon the left cheek, near the ala of the nose; the middle one equal in size to a bean. At the end of the fourth year, these tubercles were attacked with an intolerable pruritus; the largest soon after became fissured, and discharged some drops of a yellowish, acrid, and sometimes bloody serum. The disease extended at this time from below the internal angle of the left eye to the left commissure of the lips. M. Scarpa extirpated the whole of this mass, beginning his incision a little below the internal angle of the left eye, and continuing it along the left side of the nose, and around the left ala of it, stopping at a transverse line drawn below the nose. In a similar manner, a second incision commencing from the superior point, which, diverging outwardly, fell upon the same transverse line: these two incisions formed the superior triangle. Having done this, he, with the scissors, made two cuts through the substance of the upper lip, which were prolonged as far as the junction of the two preceding incisions with the already-mentioned transverse line; so that the wound was of a rhomboid figure, the lower angle terminating at the outer edge of the lip, the upper one being directly in a line with, and a little below, the internal angle of the eye.* The inferior half was united by means of the twisted suture, as in the operation for the hare-lip; and the superior half was brought into contact by means of the interrupted suture, which it was necessary to pass through the cartilaginous substance of the left ala of the nostril. Every thing went on well. The imprudence of the patient for a time hazarded the healing by the first intention; but absolute rest, rigorous diet, and the aq. lith. acet., dissipated in a few days the swelling that had been produced, and checked the suppuration of the punctures left by the pins. The subject of this operation lived several years afterwards in good health, and without the least appearance of relapse.

The Appendix contains an account of a case of a sanguineous tumour of the breast, which occurred in the practice of the translator, but which want of room compels us merely to notice.

* This description is accompanied by a Plate in the pamphlet of M. Scarpa.

Saggio Clinico sull' Iodio, e sulle differenti sue Combinazioni e Preparazioni Farmaceutiche, &c. i. e. *Clinical Essay on Iodine, and its different Combinations and Pharmaceutical Preparations; from Results obtained in the Clinical School of Padua, in* 1820–1821. By PROFESSOR BRERA. Octavo, pp. 106. Padua, 1822.

Observations on the remarkable Effects of Iodine in Bronchocele and Scrophula: being a Translation of Three Memoirs, published by J. R. COINDET, M.D. of Geneva. Octavo, pp. 32. London, 1821. Translated by J. R. JOHNSON, M.D.

(From the Medico-Chirurgical Review for March, 1823.)

Being thoroughly convinced that it is not to the discovery of new remedies, but to the improved application of those already known, through the increase of pathological knowledge, that we are to look for any important advancement of therapeutics, we are accustomed to hear the frequent announcement of fresh accessions to the materia medica, with perfect tranquillity; and if we occasionally avail ourselves of such in our practice, it is rather in compliance with fashion than from a conviction of their superiority, much less of their exclusive power to fulfil any important indication in the treatment of diseases. Indeed, we are well convinced, that modern medicine has derived infinitely more precious benefits from many physiological and pathological discoveries, of little notoriety and less pretensions—contained probably in some few unostentatious paragraphs, than from all the multifarious additions, rejections, and revivals, which have characterized our pharmacology during the last fifty years. Still it is neither liberal nor philosophical to reject unheard the claims of these new candidates for a place in the store-house of health; nor is it just to declare that all those which have been heard and tried are either useless or unnecessary. The great majority of such novelties unquestionably are so; yet it is but fair to admit that some few of these (for example, colchicum, croton, cubebs, prussic acid,) are at least deserving a place among the list of substances possessed of analogous powers. Each and all of these, it is true, we expect to live long enough to see consigned, like the other articles of present fashion, to a temporary oblivion, until resuscitated to gratify the unquenchable appetite of novelty, with the bag-wigs, red heels, hoops, or trunk-hose of our ancestors; still we feel that it is our duty, as ministers of health, and still more as medical annalysts, to make ourselves, and also our readers, acquainted with them as they respectively present themselves before us; and to hear and judge their claims and pretensions to

our regard, be they great or small. It is on this principle that we have, in former numbers of our Journal, devoted some portion of our pages to the medicines above mentioned; and in the present article we intend to introduce to our readers another new agent, that comes before us with as high pretensions, and probably with powers as great, as any of its recent precursors. This new medicine is IODINE, which has now been somewhat more than two years known to the profession, but which has hitherto obtained from us only a very brief and imperfect notice.—(Vol. II. p. 322.)

The simple substance, Iodine, was discovered in the soda derived from the incineration of certain marine vegetables, by Mons. Courtois, in the year 1813. Several of its properties were first investigated by M. Clement; but it is principally to Sir H. Davy and Gay-Lussac that we are indebted for our knowledge of the chemical habitudes of Iodine. For the history of this discovery, and the subsequent development of the various relations of this substance we must refer to the 88th, 89th, 92d, and 93d, vols. of the Annales de Chimie, and to the various scientific journals published in this country since the period of its discovery.

The first experiments on the living body with this new agent, were made by Majendie, who was led to conclude that it was not poisonous; but subsequent experiments of Orfila on animals completely established its great virulence, in certain doses, and justified its classification among the corrosive poisons. It is to Dr. Coindet of Geneva, however, that we are entirely indebted for the introduction of iodine into medicine. This gentleman reflecting on the benefits derived from burnt sponge, from time immemorial, in the cure of bronchocele,* and on the more recent discovery of similar virtues in the preparations of the common sea-weed (fucus vesiculosus;) and on the fact that iodine is common to these and other marine productions; was led by analogy to suspect that it was to *it* that the influence of these substances in curing bronchocele was to be attributed. As he lived in the midst of a population very subject to this complaint, he had an immediate opportunity of putting his conjecture to the test of experiment, and was speedily gratified by obtaining the most decisive proofs of the efficacy of the new remedy. The result of his first trials

* We are informed by Professor Brera that there is preserved in the library of St. Mark, in Venice, a copy of the works of Van Helmont, full of marginal annotations in the hand writing of our famous countryman Locke, among which is the following formula recommended for the cure of goitre:—

R. Spungiæ marinæ in carbonem combustæ uncias tres ossium sæpiæ ustorum, piperis longi, zinziberis, pyretri, gallarum, salis gemmæ, calcis testorum avorum—*ana unciam unam.* Terantur omnia simul in pulverem fin. cujus uncia semis. liquat. deglutitur paulatim *decrescente luna.*

were published by Dr. Coindet in July 1820, in a memoir printed in the Bibliotheque Universale; and this was afterwards followed by two others. These memoirs were translated into English by Dr. J. R. Johnson, and published in the form of a small pamphlet, in the winter of 1821. In his first memoir Dr. C. informs us that under the use of iodine a vast number of cases of bronchocele have been cured in a space of from six to ten weeks, and in such a way as to leave no trace of their existence; but that some of the tumours that appear goitrous resist the action of this remedy under every form of prescription; and that others are only dissipated partially, but so as to leave neither inconvenience nor deformity. He states iodine to be a most active emenagogue; and concludes by expressing his conviction "of its becoming, under skilful hands, one of the most powerful remedies with which modern chemistry has enriched the materia medica." In his second memoir, the author protests against the unjust clamour that appears to have been raised against the new remedy by the public in Geneva on the score of its dangerous effects on the living system; attributing such ill effects as may have been witnessed by others, to the incautious, indiscriminate, and popular use of it, and repeating his former opinions, strengthened by additional experience, of its great efficacy and (when properly administered) perfect safety. He here states that the neutral salt formed by the hydriodic acid and potass to which some pure iodine has been added (*ioduretted hydriodate of potass*) is the preparation most easy to manage, and is the one almost exclusively used by him. In this memoir the author enters somewhat more fully upon the effect of iodine on the animal economy, and gives some account of the symptoms developed by its continued use, or the *saturation* of the body with it, in which respect (as we shall see hereafter) it resembles mercury and arsenic. He repeats his belief that the new remedy will be found useful in amenorrhœa and other chronic diseases of the uterus; and states his having found it very successful in the cure of indolent scrophulous tumours of the glands in the neck. In his third memoir Dr. Coindet gives an account of his trials with the iodine rubbed in *externally* with lard, and states his having treated twenty-two cases of goitre in this manner, and cured more than half of these in from four to six weeks. He was led to employ the remedy externally in hopes that it would not be found to occasion the disagreeable effects occasionally resulting from its internal use, and which he seems to think owing to its local action on the mucous membrane of the stomach and bowels; and he considers that experience justified his hopes, as he says that this method "presents a sure and easy mode of employing this powerful remedy, exempt from those objections made to its internal exhibition." In this me-

moir he states his further experience of the iodine in enlarged scrophulous glands, and that his success has exceeded his most sanguine expectations. He suggests the probable benefit of a combination of this medicine with mercury in syphilitic complaints complicated with scrophula; and of the simple remedy in ovarian affections, from the analogy some of these bear to the disease of the thyroid gland.

Since the publication of Dr. Coindet's memoirs, iodine has been employed by many practitioners of eminence, as for instance, by Dr. Decarro of Vienna, Formey of Berlin, Majendie and Gimelle of Paris, Sacco and Omodei of Milan, Fenolio of Turin, and lastly, by the distinguished author of the Clinical Essay whose title is placed at the head of this article. In the hands of all of these it has been most successful in the cure of bronchocele, and has been considered by some of them as a valuable medicine in other diseases. In this country, we believe it has not had a very ample trial. We mentioned in a former number of this Journal that it had been used in one case by Dr. Kennedy of Glasgow, without success; but we learn from some communications in the Medical and Physical Journal for August and October 1822, that it has been used with great success in the case of goitrous affections common in the elevated parts of Sussex and Surry. In one instance a comparative trial of the sponge lozenges and tincture of iodine was made by a very judicious and well informed practitioner, Mr. Austin of Haslemere, and the result was in favour of the superior efficacy of the latter. In the part of the country just mentioned (Haslemere) we have the means of knowing that bronchocele prevails to a surprising extent, affecting almost every female (and scarcely any males) among the labouring classes. In these cases the burnt sponge lozenges (prepared by Shepherd of Fleet Street) are considered by the resident medical gentleman as of almost specific efficacy, if persevered in for a sufficient length of time, and are not found to be productive of any ill effects.

Mr. Austin, we are informed, is now engaged in giving a full trial to the iodine; and we have every reason to expect from him a judicious administration of the remedy, and a faithful history of its effects.

It was with the knowledge of the results obtained from the employment of this remedy in France, Switzerland, and Germany, that Brera determined to give it a full trial in the Clinical School of Padua in the year 1821; and in the small volume before us he has submitted to the profession the fruits of his experience. Although the author entitles this a *Clinical* Essay, and commences with a detail of cases, yet he takes occasion, in the course of his memoir, to give a more connected and comprehensive view of the general medical relations of iodine than is

to be found in any other work; and as we think these details will prove much more interesting than the cases, we shall reverse the order of his Essay, and give an account of these general matters first. In doing this, as our object will be to submit to our readers an epitome of what is known respecting the new remedy, we shall neither restrict ourselves to the order nor to the substance of our author's treatise, but cull our materials from whatever other sources are accessible to us.

I. *Chemical History of Iodine.* For a complete accoun t of this we must refer to the sources formerly mentioned, and to the work before us. We shall only here observe that iodine is a simple substance, of characters analogous to those of oxygen, chlorine, &c. solid at the ordinary temperature of the atmosphere, but volatile at a moderate degree of heat, under the form of beautiful *violet-coloured* fumes, from which it has derived its name. It is very sparingly soluble in water, but much more so in alcohol and æther. It is not inflammable. It forms acids when combined with hydrogen, oxygen, and chlorine, which are respectively named—the *hydriodic*—the *iodic*, and the *chloriodic* acids; and it unites with many of the metals forming *iodurets*. Its acids form salts with the alkalies, earths, and metals; of which, and also of the pharmaceutical preparations derived from these, a very complete catalogue is given in Brera's memoir.

II. *Pharmacology.* We have already stated Dr. Coindet's opinion that the ill effects occasionally found to follow its internal use would be obviated by its external application. Brera, however, informs us that further experience has proved the fallacy of this notion, and assures us that it can be employed internally with equal safety, and with greater effect, except in such cases as require its topical agency. The following are the formulæ most recommended by professor Brera.

1. *Tincture of Iodine.* Made by dissolving 48grs. of pure iodine in an ounce of alcohol (at 35.) This is the preparation most frequently used at first by Dr. Coindet, who, as well as Brera, recommends it being used *fresh*, as it is liable to decomposition in a few days. The dose is from five to twenty drops for adults, three times a day. Twenty drops contain about one grain of iodine.

2. *Pills of Iodine*, made by forming one grain of iodine into two pills, with elder-rob and liquorice powder—one to be taken morning and evening.

3. *Iodine Ointment*, made by rubbing up a drachm of pure iodine with an ounce of lard, or half a drachm of hydriodate of potass with an ounce and a half of lard; the former in the quan-

tity of a scruple, the latter about the size of a filbert, rubbed on the part.

4. *Solution of Hydriodate of Potass.* This preparation is stated to be preferable to any of the foregoing, producing their good effects without their inconveniences. It is formed by dissolving 36 grains of the hydriodate in an ounce of distilled water, and is given in the same dose as the tincture.

5. *Solution of the Iodurettted Hydriodate of Potass*, formed by dissolving 36 grains of the hydriodate and ten grains of pure iodine in ten drachms of water. This is said to be a still more efficacious preparation than the preceding, and requires to be given in small doses, viz. five or six drops, three times a day, to begin with.

The following precautions are to be attended to during the administration of iodine:—not to combine it with substances likely to decompose it, and not to give it when the stomach is loaded, but in the morning, a couple of hours before or after dinner, and in the evening. Our author farther recommends the occasional suspension of the medicine, on account of the sometimes sudden supervention of unpleasant effects from it, and to give a dose of magnesia on the day of its suspension, with the view of clearing the primæ viæ. The liquid preparations may be given in any vehicle. Coindet usually employed syrup and water.

III. *Effects of Iodine on the Living Body.*

A. *On Animals.* We have already observed that both Majendie and Orfila, on the first discovery of iodine, made experiment of its effects on animals. The following are the results derived from the trials of the latter on dogs.

1. Introduced into the stomach in small quantity, it acts as a gentle stimulant, and excites vomiting.

2. In the dose of a drachm it invariably destroys the dogs to which it has been administered, (the œsophagus being tied,) producing ulceration of the mucous membrane.

3. In the dose of two or three drachms it produces similar effects in the animals whose œsophagus has not been tied, provided they have not vomited for some hours after its ingestion.

4. It is not fatal when applied externally.

5. It seems to act on the human body in the same manner as on animals.

6. It ought to be classed among the corrosive poisons.

B. *On Man.* When iodine is cautiously and gradually introduced into the system, it affects it in a general manner, ana-

logous to that of mercury, but very different in the consequences. The first, and what may be called the *salutary* effects of iodine, are an increase of appetite and of the strength of the pulse; whenever these are produced we must watch with the greatest care that these salutary limits are not exceeded, and the pernicious consequences of an over saturation of the system induced. The complete impregnation of the system is indicated by the change of the above-mentioned increased action of the pulse into decided frequency and quickness—by a sense of heat and irritation of the fauces—pain of the orbits or eye-balls, with obscured vision—pain of the internal ears and gums, (with occasional salivation,) headache, restlessness, loss of sleep, with swelling and pain of the diseased organs, (*e.g.* thyroid and other glands,) and an increase of appetite sometimes to a degree of voracity. In some persons the submaxillary glands become painful and swollen, and a similar state of the mammæ, *with eventual diminution of their natural volume*, takes place in some females. When given from the first in an over-dose, iodine produces a strong burning sensation in the fauces, which frequently extends down the œsophagus to the stomach and whole intestinal canal. In a still higher degree of saturation (or *iodization*, as the author calls it) of the system, to the abovementioned symptoms succeed very considerable emaciation even in the space of a few days, excruciating pains in the orbits and eyes, with great defect of vision, and similar pains in the diseased parts; the strength vanishes; neuralgic pains are experienced in the stomach, chest, bowels, &c. the sleep entirely fails, and there is obstinate palpitation of the heart, with tremors, convulsions, or palsy of the extremities; to the excessive appetite succeeds complete anorexia, and the factitious disease finally terminates life, in a short time, by universal inflammation of the nervous and vascular systems, (profonde angioitidi e neuritidi.)

Upon the appearance of the milder class of symptoms abovementioned, the immediate suspension of the medicine (which ought always to be done) sometimes is found sufficient to put a stop to them in a few days. For allaying these deleterious effects, rigorous regimen, copious mucilaginous drinks, and the tepid bath, are recommended; and where the topical affection of the goitre, or other tumours, runs high, fomentations, poultices, leeches, &c. are prescribed; and general bleeding is advised where there exists a high phlogistic state of the whole system.

As these symptoms sometimes show themselves all at once, we ought to be cautious in not too hastily increasing the dose in cases wherein no obvious effects are produced. After the bad symptoms are allayed, the medicine is to be repeated with the same precautions as in the case of mercury and arsenic.

IV. *Medical Effects—or Effects on Disease.* From the preceding history of the effects of iodine on the living system, it must be admitted to be a powerful agent; and the statements formerly made of its efficacy in the cure of bronchocele, prove its potency to be available in the hands of medical men. Of its precise mode of acting on the living system, more especially in the cure of disease, we are hardly well assured. Dr. Coindet says,

"Iodine is a stimulant; it gives tone to the stomach and excites appetite; it neither acts upon the bowels nor kidneys; produces no perspiration; but exercises its action upon the generative system, especially the uterus. If given in a certain dose, and continued for some time, it is one of the most active emenagogues with which I am acquainted; it is, perhaps, from this sympathetic action that it so frequently cures the goitre."—*First Mem. p.* 12. Again—"The experience of two years upon more than two hundred patients, has proved to me, that this remedy is one of the most energetic stimulants we know of the lymphatic system; and the variety of diseases in which I have employed it, (such as goitre, scrophula, enlarged glands of the breast, certain affections of the uterus, some cases of dropsy, &c.) is only apparent, since the whole of these diseases are only lesions of the same system."—*Third Mem. p.* 31.

Professor Brera sums up his opinions respecting the new agent in the following terms:—

"Iodine, then, is, on many accounts, entitled to be classed among the *heroic* remedies, and to obtain a place by the side of mercury. Like mercury, it maintains a permanent action on the system for a considerable time after its administration has been suspended. Powerfully exciting the nervous system, it accelerates the action of the heart and arteries, and restores the functions of the sanguiferous and organic systems when preternaturally affected. It thus produces appetite, fattens the lean, and emaciates the robust.(!) Determining a particular action on the thyroid gland and uterus, it removes the morbid enlargements of the former, promotes scanty, and lessens excessive, menstruation, and even diminishes the size of the mammæ This last mentioned fact, taken in conjunction with its undoubted efficacy in removing goitre, gives us reason to hope that this medicine may be found beneficial in the cure of organic enlargements of the ovaries and uterus."—*Saggio, p.* 83.

He afterwards adds, that iodine produces its remedial effects without exciting any purgative, diuretic, or diaphoretic action; and that it has no effect on the thyroid gland and uterine functions, when in a *sound* condition.

Many of these sanguine anticipations of the wonders worked,

or hereafter to be worked, by iodine, we have no manner of doubt will be disappointed, and are, in truth, little justified by the facts adduced by their very promulgators. Of the great powers of iodine in the cure of bronchocele, we think, we cannot, from the evidence before us, legitimately doubt; and the establishment of this single fact is sufficient, in our minds, to entitle the remedy to the greatest consideration. So rarely, indeed, are we presented, in the practice of medicine, with agents of specific powers, on whose efficacy, in the removal of disease, we can calculate with any thing like certainty,—and so frequently are we left in doubt, even in our most successful cases, whether it was our prescriptions or the spontaneous agency of Nature's restorative powers, that produced the benefit,—that it invigorates, at once, our confidence in medicine, and our zeal in the practice of it, when we fall upon a remedy on which we can pretty confidently rely, even, although the sphere of its operation be confined to a single form of disease. If, then, it be true, that *iodine*, whether in the form of burnt sponge, or kelp, or under a more scientific aspect, is capable of removing a considerable proportion of goitrous tumours, even of many years' standing, we shall be willing to give it a high place in the materia medica, even if its powers should be entirely confined to this class of affections.

In the memoir before us, Professor Brera does not detail his experience of the efficacy of iodine in bronchocele, because, he says, the results obtained by him have been similar to those of Coindet and others. His principal object appears to have been, to try its powers in some other forms of disease, in which it had been proposed or employed by its discoverer, or suggested, to himself, by analogy. The clinical observations detailed by him, are thirteen cases of his own, and four communicated by one of his friends, and consist of one case of (supposed) incipient tabes mesenterica; two cases of vicarious hæmoptysis, and one of vicarious hæmorrhage from the eye; one of chronic dysentery; one, laryngeal phthisis; three of amenorrhœa, and one of dysmenorrhœa; two of swelled submaxillary glands; two of bronchocele; two of scrophulous ophthalmia; and one of scrophulous glandular tumours.

In reviewing the history of these cases, as given by our author, we are compelled to declare, that, in as great a proportion as two-thirds of the whole, there is no proof whatever of the complaints being removed by the iodine; and, that in several, it seems infinitely more probable, that the other means employed at the same time effected the cure. Indeed, we are extremely surprised to find these cases adduced by a practitioner of Professor Brera's experience, as instances of cures effected by the iodine. In the cases of bronchocele, however, and other en-

largements of glands, the effect of the remedy appears to have been sufficiently conspicuous; and, in conjunction with Dr. Coindet's experience, fully justify its employment, and with considerable prospect of benefit, in indolent glandular tumours in strumous subjects. We have already devoted too much of our space to permit us to give even an outline of these cases. We must, therefore, refer to the work itself.

In conclusion, we would beg leave to repeat our opinion, that in iodine we have a valuable addition to the materia medica; that its efficacy in the cure of bronchocele, appears to exceed that of every other medicine hitherto employed; that its power in dissipating other chronic glandular tumours seems very probable; and, that it is deserving of a trial in other diseases, more especially in such as bear any analogy to those in which it has been proved to be beneficial.* At the same time, we must recur to the declarations with which we set out, and which can never be too much and too frequently impressed upon the minds of the younger members of the profession,—that it is only by the *study of* DISEASES that professional eminence or usefulness can be obtained,—that a constant hunting after, and great confidence in, medicines of supposed specific powers, is an unfailing indication of a weak understanding,—and, finally, that, according to the expression of the great Boërhaave, *there really is no remedy* "*nisi quod* TEMPESTIVO USU *fiat tale.*"

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1. *A complete Treatise on the Nature, Symptoms, and Cure of Lues Venerea, Historical, Theoretical, Practical, and Original. A new Edition, amended and corrected.* By JESSE FOOTE, Esq. Surgeon. London, 8vo.—pp. 420.
2. *Facts illustrating the Effects of the Venereal Disease on the Fœtus in Utero, and the Modes of its Communication.* By WILLIAM HEY, Esq., F.R.S., &c. &c., of Leeds; communicated in a Letter to JOHN PEARSON, Esq., F.R.S., &c.; read June 25th, 1816. Medico-Chirurgical Transactions, vol. vii. p. 2.
3. *Remarks on the Opinions of Mr. Hey, by Mr. Pearson. Appendix No. II. to Life of William Hey, Esq.* By JOHN PEARSON, Esq., F.R.S., &c. London, 1822.
4. *Dictionnaire de Médecine, Tome V.* Paris, 1822. *Article Chancre.* By L. V. LAGNEAU, M.D.

(From the London Medical Repository for April, 1823.)

The researches of the indefatigable Astruc resulted in the conviction that lues venerea was a new disease, which first made

* Since writing the above, Dr. Baron's "Illustration" has come to hand, and in it we find strong reasons for hoping that, in iodine, we possess a medicine of

its appearance in Europe about the year 1494. He found that, immediately after the above period, writers expressly and exclusively treating of this disease were numerous, and that the greatest unanimity prevailed amongst them in the opinion that it had newly appeared in Europe, and was totally unknown to their predecessors. His zeal in the inquiry after the origin of lues led him to compose a chronological index, including a brief analysis of the works of authors who had written on that disease exclusively, and carrying it down to the end of the seventeenth century. He was unable to discover a single work treating separately of lues, prior to the period above named, and whatever apparent allusions to its symptoms were to be found in older writers, were so indistinct and questionable, so inconsiderable in amount, compared with the full narrations of writers after this date, that the conviction affected him irresistibly.

Since the time of Astruc others have engaged in the same search, and some have arrived at very different conclusions. Collecting from all ancient authors every expression which could be supposed in any way to allude to individual or aggregate symptoms of the disease, they have amassed a pretty considerable bulk of quotations, which they seem to think sufficient to prove satisfactorily, that it is a very ancient disorder, which has prevailed in all countries since the origin of history.

Equally discordant opinions have been entertained respecting the distinguishing characteristics and the most eligible plans of treatment; so that the subject has long been held one of the most complex and difficult in medical science.

Among authors of our own country, who entertain opposite opinions upon the historical question, but who are far less at variance on the subject of treatment, two are now living, who many years since published works upon the disease, and, after a period of more than thirty years having elapsed from the first publication, have within a short time reissued these works for the examination of the rising generation of surgeons. Dr. Swediaur and Mr. Jesse Foote have, perhaps more than any of their cotemporaries, devoted themselves to the investigation of the history, nature, and cure of syphilis, and have, for a longer period than falls to the lot of many inquirers, had extensive opportunities of observation. The work of the former has been reviewed in the REPOSITORY, and we now propose to analyse that of Mr. Foote.

Great prejudices have been very generally entertained against the writings of this gentleman, principally, we believe, originating in the observation of his hostility to the late Mr. Hunter,

no mean power in resolving tubercles in the lungs, and tubercular affections in other parts of the body. We refer our readers to the analysis of Dr. Baron's work in another part of this number for further particulars.

whose popularity has been so great, that to question the accuracy of his opinions has been to incur a species of odium with the great bulk of the Profession in England. We shall not be accused of disrespect for our revered pathologist, however, in endeavouring to make farther known some very ingenious opinions of one whose feeling of hostility must now have ceased, and who seems to come forward to republish those opinions because he believes them likely to serve his fellow men.

No two men could differ more widely than did Mr. Hunter and Mr. Foote, especially on the subject of the work before us. In the outset, however, their opinions are little at variance. Mr. Foote, following Astruc, is of opinion that the venereal disease, pre-eminently so called, otherwise lues venerea, or syphilis, is a disease of modern origin in Europe, that it was imported by the discoverers of America, conveyed by the Spanish soldiers to Naples, and dispersed over the greater part of Europe by the soldiers of different countries, assembled at the siege of that place. Mr. Hunter believes it to be of modern origin in Europe, but does not attempt to determine whether or not it came from America. Dr. Swediaur has considered the subject more fully than the latter, but, in our opinion, he is by no means successful in disproving the modern rise of this disease. He takes great pains to prove that it was known a few years prior to 1494. But supposing that it were not imported from America, but might be introduced from Africa by the Moors and Spanish Jews, we still have abundant proof that in Europe it had not been long common, for prior to 1494 not a single volume had been published on the subject, but between 1494 and 1536 many works, expressly and exclusively treating of it, were published, and the writers of these works uniformly speak of the disease as new.

Quitting, however, the historical question, though by no means considering it as trivial, or unworthy of being fully canvassed, we pass on to some points on which our readers will find greater satisfaction if their doubts and perplexities can be cleared or removed.

"Lues venerea," says Mr. Foote, "appears to be a poison *sui generis*, a poison peculiar to the human subject;" and farther, "there is no disease which ever was defined, so full of variety as lues venerea."—"In order to account for the venereal action on the various parts, both local and constitutional, we must consider the quality of the parts on which it is acting; we must consider that venereal infection has the singular power of acting on every component part of the body."—"It is owing to the uncommon subtilty of venereal poison, and the power which it has of attacking every component part of the body, to the *irregular* action of the poison on each part, in point of *succession*, to the length of time it will re-

main without discovering itself, to the progress of it not being definable under any *fixed periods*, to its assimilating on various parts to other diseases which such parts are liable to, that so many disputations have engaged authors, so many erroneous opinions have been given, such various theories have been adopted, such contradictory practice in the treatment of it has been suggested, such false conclusions drawn, and such impositions on credulity practised."

Having premised these remarks, he goes on to state his opinion, that—

"Gonorrhœa and chancre are both the result of venereal poison acting on parts, under different modifications." "The cause of both symptoms," he says, "is the same, and the effect will be corresponding to the anatomical nature of both parts: venereal fluid, applied to the urethra, produces a discharge of mucus; that fluid, lodged on the cutis, produces a chancre."

The history of the disease militates somewhat against this opinion, for Fallopius asserts that the gonorrhœa was unknown as a symptom of lues, till about the year 1545; and, according to Astruc, all writers upon the disease, whose works appeared before that time, are silent respecting this symptom. Again: the quotations which have been brought in evidence of the antiquity of the venereal disease refer to an affection having some of the characteristics of gonorrhœa.* Mr. Foote, however, explains away these opposing circumstances, by observing, that Surgeons, when the venereal disease first appeared, must have been familiar with discharges from the urethra and vagina, arising from other causes, and that this very familiarity prevented their perceiving, at once, the connexion between this symptom and a venereal cause.

"And as they did not at first annex the symptom through ignorance, so did they afterwards annex it through information. If they had first of all annexed it through ignorance, and afterwards rejected it through information, then the case would have been widely different: for it would have been apparent, they were apprized of the fact from the very beginning, had weighed it, and had decided to reject it. But from the very first time that gonorrhœa was adjudged to be an increased discharge, flowing from the urethra, in consequence of venereal infection, and was considered as true a symptom on that part as a chancre was on another part, that fact has not, until very lately, been resisted. In the whole of the historical tracts on lues venerea, which have been written year after

* William Becket, in vols. xxx. and xxxi. of Philosophical Transactions, quotes several English surgical writers to this purport, scraps from numerous foreign authors have been collected by others, and even the Bible has been referred to in support of the opinion.

year, that fact, so far from being doubted, has been always established."—P. 39.

Various experiments have been instituted to determine or to disprove the identity of the poison producing gonorrhœa and chancre. They have been performed upon a small scale only, by individual experimenters, and each seems to have arrived at the conclusion he anticipated. We shall see presently how Mr. Foote explains their failing to be satisfactory; why he appeals to observation of the natural occurrences of the disease as the best source of accurate opinion, and how strikingly recent observers have confirmed his remarks.

After a caution to Practitioners not to decide upon the innocence of a discharge, the effect of protracted gonorrhœa, and a little sarcasm or two on the loose opinions of Mr. Hunter respecting the question of when infection need be feared, he examines these experiments and their results, first defining gonorrhœa to be "a virulent discharge of fluid from the urethra in man, and vagina in woman;" and chancre, "an ulcer on any part of the penis of a man, or labia of a woman; or on any other part of the cutis or cuticle, where the venereal fluid, which is necessary to infect, has been so lodged as to produce that effect."

The question is:—

"Whether the *infecting discharge* from the *urethra* is capable of producing a *chancre* on *another subject*—or whether *chancrous* discharge is capable of producing a *gonorrhœa* in *another subject*—or whether a subject, having a *chancre*, can infect *another* with a *chancre and gonorrhœa*—or whether a subject, having a *gonorrhœa*, can infect *another* with a *chancre and gonorrhœa?* Whether these *two* symptoms be produced from virus, the *same* in its nature, but only acting on two different anatomical parts—or whether the virus which produces them be not of a *different* nature, so that gonorrhœa from one cannot produce chancre on another; nor can chancre from one produce gonorrhœa on another."—pp. 43 and 44.

One experimenter applies the matter of gonorrhœa to the glans penis, and produces no chancre, but another succeeds in producing a chancre by the same application. A similar disparity of results attends the application of chancrous matter to the lining of the urethra. Mr. Foote observes, "We are not told by them whether the experiment was made on the *same* subject or on *others*," and he afterwards contends, that the matter of the *same* subject will not act upon that subject to produce either symptom, agreeably to a law hereafter to be considered. The experiment may fail, too, from various causes, and its failing even many times will prove nothing; but if it once succeed, the question is at an end, provided the accuracy of the observa-

tion be duly attested. Mr. F. goes on to examine the natural evidence of what each matter can produce, and from observation of the appearance of chancre on men who had been infected by women having gonorrhœa only, is satisfied that this matter can produce chancre, and that the cause is identical in the two varieties of local symptoms. The confirmation of his opinions to be derived from recent observations is afforded by a work of Dr. Hennen (Principles of Military Surgery), in which, after remarking that "soldiers are gregarious in their amours," and that, of several connected successively with the *same* woman, "*some have had one kind of sore, some another, and some both,*" he gives in a note the following curious case, from Vigarous:—"Of six young Frenchmen who had connexion successively with the same woman, the first and fourth in the order of connexion had chancres and buboes, the second and third gonorrhœa, the fifth chancre, and the sixth bubo;—[Vigarous, Oeuvres de Chirurgie-Practique, Civile et Militaire, Montpellier, 1812. Complication du Vice Vénérien, page 8;"—] and adds, "I have had an instance of three individuals similarly circumstanced; the first escaped, the second had true chancres and elevated sores, the third had gonorrhœa. The connexion took place within an hour."* Mr. Evans, too, relates one case, at least, in which the sore, which he terms venerola vulgaris, and gonorrhœa, were successively produced in the same gentleman by intercourse with the same woman; and remarks: "In this case, then, we see *both* venerola vulgaris and gonorrhœa take place in the same person from the same cause."† We observe, by the way, though out of place, that this said venerola vulgaris is followed by secondary symptoms *occasionally;* that it occurs in so many "*spurious forms*" from various causes; that its "*gradations*" are from "*the purer form,*" particularly described by Mr. E., "*downwards, until* ALL *its peculiarities are lost in the superficial ulcerations ranked under the head of excoriations;*" and that on the effects of mercury depends almost entirely its diagnosis.

A difficulty presents itself to Mr. Foote which has occurred to others. If the poison producing chancre and gonorrhœa be the same, why is it that a man may have a chancre without a gonorrhœa, and a gonorrhœa without having a chancre?

To illustrate the law of the action of the venereal virus by which the difficulties in the way of his opinion being substantiated are chiefly removed, Mr. Foote states the order in which several primary appearances of the disease occur, influenced by the character of the parts on which they appear.

* Hennen's Military Surgery, p. 526.
† Evans on Ulcerations of the Genital Organs, p. 80.

"When," he says, "a person has contracted both symptoms from the same connexion, gonorrhœa will be the first to betray the infection—the corona glandis, frænum, and the internal part of the prepuce, will be the next—and the cutis, or the external part of the prepuce, will be the last. I have known the space of time between the appearance of gonorrhœa and that of chancre on the cutis of the penis to be three weeks; and yet as there was but one connexion to produce both, I am confident the one did not arise out of the other; but the true cause of the distance of periods between them was, that it would take but a short time for the venereal stimulus to produce a discharge from the urethra, and a longer time for the same stimulus to produce a chancre on the cutis; and also, that it would take an intermediate time between the former and the latter for the same stimulus to produce a chancre on the glans, internal part of the prepuce, or frænum.

"When all these symptoms do appear from one infection, such is the general order of their appearance: I say general, because I do not suppose but there might be exceptions. It is also very well known to those whose practice affords them the opportunity of obtaining that knowledge, that a gonorrhœa is the most general first symptom, when it comes on alone; that chancres on the frænum, and internal part of the prepuce, are the next most in common; and that chancres on the external part of the penis, and parts adjacent, are the most uncommon. Why this difference is found, it is not owing to the virus from the woman not coming into contact with the one part more than the other, but to the difference in the construction of those various parts—to the more susceptibility of the one than the other—to the one being more disposed to be stimulated by the virus than the other—to the one being capable of being acted upon by the virus when the other may not—to the virus being lodged and remaining undisturbed—to the faint power which the virus might at the time possess when the infection was given—or to the difficulty there is in the subject, who is infected, to be infected.

"For if a man be connected with a woman who has a gonorrhœa, every part of the penis may be presumed to take up the moisture; but as the cutis is more difficult to be infected than the cuticle, and the cuticle than the urethra; and as the fluid lodging on the external part of the penis is more liable to be wiped away, so does it happen that that part is the most exceptionable. The urethra being a part liable to be stimulated by a stimulus which would not stimulate the skin, is, of course, liable to be stimulated by a power which in its nature may be too faint to stimulate or act on the prepuce: besides, the natural formation of the urethra prompts it to throw off any thing which is obnoxious to it; or, at least, it is readily prompted to discover whatever produces an irritation upon it: and therefore, also, it might very naturally be inferred, that if there be stages of the gonorrhœa when the power of the infecting fluid is weaker, and less active, the urethra, from its nature and construction, will be liable to be acted upon by it, when other parts cannot."—pp. 53 and 54.

By the power of the virus being faint, Mr. F. means, that if a chancre be nearly healed, or a gonorrhœa be past its height, the matter produced by either has, he believes, lost its virulence in part, and will be incapable of affecting with the disease the parts most difficult to affect, yet will retain power to act on the urethra or other part of next degree of susceptibility. That morbific poisons undergo considerable changes, corresponding to the period of the disease in the subject from which they are derived, cannot be doubted, since vaccination has put it in the power of every Practitioner to make the remark; and the history of vaccination abounds with proof of its accuracy. Yet though the power of the matter to infect may not be strong, if it does infect, no material modification of the disease is occasioned by that circumstance. If capable of producing the disease at all, it produces it in its usual form, and of ordinary severity; but the capability to produce may vary. The severity of the disease will depend upon the constitution of the recipient, not upon the condition of the infecting matter.

Mr. Foote having thus shown how one appearance may succeed another, all arising from the same exposure, proceeds in the illustration of his opinion that it is a law of the venereal poison (in common, he seems to think, with other poisons, but certainly not so):* that the matter of the same individual will not produce fresh local symptoms of the primary characters upon himself; that gonorrhœal matter, for instance, will not produce chancre upon the person affected with that gonorrhœa nor chancre gonorrhœa in him affected with that chancre, nor chancre produce other chancres in the same individual.

"It has been said, and very truly said, that if a gonorrhœa and chancre be found upon the same subject, they do not in their progress interfere with each other; that they appear to be so very distinct, and require such a different treatment, that those who at first suspected them to have not originated from the same virus, will yet, from these observations, have their suspicions more strongly confirmed. If a subject has a gonorrhœa only, I am of opinion that the gonorrhœa will not produce a chancre on the external part of the

* Vaccine virus, taken before the sixth day, will produce new vesicles on the person from whom it is taken, and these will be expedited in their progress through the usual stages as the constitution becomes influenced from the original insertion. Yet though this be an exception to the law of infection which Mr. Foote aims to establish, it is one which ultimately gives support to his opinion, as of general application; for it would appear, that whenever the infecting matter is by nature unconfined to the part in which it is generated, it does not possess the power of infecting anew the individual producing it; but that such a power of infecting is possessed by matter which, as the vaccine within its vesicle, is naturally confined. So that a natural provision against the inconvenience is made where required, and is not required where it is not made.

penis, nor within the prepuce. I will go farther: I am confident that a chancre is never produced from a gonorrhœa of the same subject; and I am also confident that a chancre on the penis will not produce a gonorrhœa on the same subject. This may, in the opinion of some, be still widening the difference in the nature of the two fluids, which, according to my theory, ought to be united. I will go farther, and say, that I have seen a chancre destroy almost the whole of the *glans penis*, without bringing on a gonorrhœa; and I have seen a gonorrhœa produce a swelling of the penis—an excoriation and discharge upon the glans—an abrasion of the whole surface withinside of the prepuce, without there being the least appearance of a chancre.

"If the virus from a chancre could produce chancres on the same subject, then chancres would be found wherever the fluid touched and lodged on a sound part, and the whole of the glans would become a galaxy of chancres. I will state a case for the illustration of my argument. A gentleman that I was attending at the time of my writing this, first of all perceived a chancre on the frænum, and another about the eighth of an inch from the frænum. He had discovered both about three days. On the fourth day, after he applied to me, a pimple was discovered on the dorsum penis, which was observed for three days before the truth of its nature was ascertained. This also proved to be a chancre, and the late appearance of it after the two former was most assuredly owing to its being on the skin, as the two former were more early, by being on parts not clothed by skin. They were all three contracted at the same time. These three chancres never produced a fourth. The two first could not possibly produce the third. The third was quite out of the way of the fluids of the other two; besides, it was discovered too soon after [for]* the other two to be produced by them. I cannot take upon me to say whether the woman who infected this gentleman did it through gonorrhœa or chancre; but I will give my reason why I think he was infected by gonorrhœa and not by chancre—because two of the chancres were about the frænum, and the other at a distance from them on the *dorsum penis*. One chancre could not have produced this infection from the woman—there must have been more—but the fluid from gonorrhœa could effect it with facility."—pp. 55 and 56.

In treating of *Gonorrhœa* by itself, Mr. F., unlike some of his cotemporaries, does not choose to employ any new term, though this be not etymologically correct, to designate what every body well knows by the appellation; saying "a word can only convey the name of a thing, but never the full meaning or description of it; and, therefore, if we do but know the thing by a name, that is all we want." Having discussed the nature of the discharge, which he considers to be mucus, *i. e.*

* We enclose this word between crotchets, believing it to be unnecessary to the sense, which to us it obscures.

the natural secretion increased in quantity and somewhat altered in quality, but still formed by the same parts, and after the same manner, just as saliva, in a spitting from mercury, is still to be called saliva, though differing in most of its sensible qualities from the ordinary fluid of that name, he then enumerates and clearly describes the symptoms, in the usual order of their occurrence, comparing the effects of inflammation arising from this specific cause with those produced in inflammation of ordinary origin in mucous membranes. This we consider a very useful view of the subject; for though the cause, in the one instance, has generally a more permanent operation than in the other, the effects bear a close resemblance in character, and are also alleviable by similar means. When in high degrees of inflammation of the lining of the air passages, for instance, instead of an increased secretion, no discharge whatever is yielded, antiphlogistic measures are more imperatively demanded; and so in gonorrhœa, if the discharge suddenly ceases, and especially, as is often the case, if retention of urine be attendant, it will frequently be advisable to bleed the patient actively, as well as to employ the more common measures of depletion. Some remarks follow on the *specific* distance of Mr. Hunter. This was one of the points of difference between the opinions of Mr. Foote and Mr. Hunter. Mr. F. considers that, to whatever extent the lining of the urethra is affected in gonorrhœa, to that extent is the inflammation specific, but beyond the length of that membrane some of the symptoms may be the effect of that specific inflammation of the mucous membrane. The state of the case appears to be this. Inflammation is produced in a mucous membrane by a specific cause, which empowers or compels that membrane to furnish a secretion capable of exciting similar inflammation in the mucous membrane of another subject. That, in mild cases, the extent, it would appear, to which the membrane is excited to the production of such infecting secretion does not exceed two inches or thereabouts; but there is every reason to believe that, in other and more severe instances, a more extensive portion of the membrane is inflamed to the degree of furnishing an increased secretion, which no one can prove to be incapable of producing gonorrhœa in another subject, and which, as there is no natural line of demarcation, nor any apparent barrier to the extension of effect, nor any visible difference of quality or office of parts, which could in any way account for such a difference as has been supposed to exist, must by plain-thinking men be presumed so capable; and that therefore the other parts of that membrane must be included within the bounds of parts affected specifically, when at all affected by the disease.

We extract Mr. Foote's "*theory of chordée*" as a specimen

of his ingenuity in explaining the phenomena of the disease to which he has paid so much attention:—

"The chordée is certainly produced from an inflammation of the substance of the urethra, from the inflammation having penetrated beyond the surface, and affected the corpus spongiosum urethræ. The corpus spungiosum urethræ being inflamed, becomes painful and tumid, the circulation is impeded in it, and the power of extension restricted. The corpus spongiosum urethræ, in a state of health, confirms and corresponds in action with the corpora cavernosa penis. When the corpora cavernosa penis are in a state of flaccidity, so is the corpus spongiosum: and when the blood is thrown into the corpora cavernosa, so as to increase their bulk, and cause thereby an erection of the penis, the corpus spongiosum also receives its share of sanguineous aid, and completes the intention. This is often the effect of the will, often the consequence of venereal reflection, sometimes an act from the heat of the bed, from an inflammation of the part, and from a stimulus on other parts which correspond with the penis. It is a venereal stimulus from a mental idea which produces an erection in a state of health—and it is an inflammatory stimulus of the parts which produces the erection in a state of disease.

"In this instance of cordée, the corpus spongiosum urethræ is so highly inflamed, that it cannot bear distention, and it is the act of distention which produces the pain that is felt; for the corpora cavernosa, not at the same time inflamed, are capable of their usual distention; but when the blood is flowing into the reticular substance of the corpus spongiosum, the vessels of it are found to be inflamed, tumid, and too much loaded; the effort, therefore, cannot be even attempted, without producing great pain, and without sometimes lacerating the reticular substance, from which a considerable hæmorrhage ensues, and which is, in general, found to be a relief. It is when the corpora cavernosa are swelled with blood that the compression of them upon the corpus spongiosum produces a feeling as if the penis was strongly squeezed between the finger and thumb. This is my theory of chordée."—pp. 76, 77.

Phymosis from gonorrhœa, *paraphymosis*, and *swelled testicle*, are next treated of, and Mr. Hunter is accused of having omitted to mention the two former as occurring in consequence of gonorrhœa. Mr. H. has, indeed, placed the account of them which he gives among the occurrences which are effects of chancre, and as such they certainly often enough present themselves; but he has expressly stated that they do happen "sometimes in the common gonorrhœa," as well as in that of the glans and prepuce, and with chancres, "but most frequently of all from a chancre in the prepuce." Both these gentlemen, in more or less direct terms, speak of these symptoms as inflammatory, but neither of them, in our judgment, sufficiently considers that they ought to be treated simply as inflammatory. Mr.

Hunter's practice has the faults of the surgery of his day, which, looking upon every symptom of the disease as requiring the application of the specific, mercury, never employed sufficiently active measures to reduce the attendant inflammation.* Mr. Foote's is certainly a considerably improved practice. He bleeds freely in the arm if the case be severe, or with leeches if mild, uses a spirituous fomentation and poultice, to which spirit of wine has been added. He also *forbids* what Mr. Hunter *recommends*, the washing of the inflamed prepuce and glans with the patient's own urine in the act of voiding it. Both of them seem much too ready, when chancre exists, to commence the introduction of mercury. Upon the whole, however, Mr. Foote's practice much more nearly approaches what we humbly conceive to be the best, that of treating the affection, till the swelling is upon the *decline*, as purely inflammatory, especially by the employment of spirituous lotions with rags to envelop the whole penis, and the injecting of warm water or milk and water beneath the prepuce, to wash out the discharge and dilute every drop of urine which may unavoidably lodge upon the parts. To this end Mr. F. directs that "the patient should always urine with the penis in warm water, to wash away the urine confined within the prepuce."

Swelling of the Testicle.—The period and the progress of this symptom are very distinctly described, but it is unnecessary for us to enter upon those particulars. We pass over them and Mr. Foote's strictures on the word sympathy as employed by Mr. Hunter. From observing that one testicle only is in general affected, and that even in very severe cases of gonorrhœa, the inflammation of which has extended the whole length of the urethra, not uncommonly the testicles both escape, if the patient be guilty of no imprudence, Mr. F. thinks "there is a difficulty in the effects of a swelling of the testicle being brought about."

"I am, therefore, of opinion, that the orifices of the vasa deferentia, which open into the urethra, are shut against the effects of all stimuli; and that it is from accident alone venereal stimulus can possibly be admitted. But if it once gain admittance, I am of opinion that the venereal stimulus can act as well along the vas deferens, as it does along the urethra; and that it is the positive presence of the venereal stimulus which produces an inflammation on the internal canal of the vas deferens, and which proceeds to the epidydimis, and affects that and the testicle. If virus can pass through lymphatics, by what is termed absorption, there is no diffi-

* On this subject we recommend to our younger readers the perusal of Mr. Travers's essay on phymosis and paraphymosis, in the first part of his and Mr. Cooper's Surgical Essays.

culty to presume that it may pass along the vas deferens by capillary attraction. And I am also of opinion, that part of the discharge which follows a swelled testicle, and to which a swelled testicle from a venereal cause owes its restoration to a sound state, flows through the vas deferens, and that it is poured from thence into the urethra.

"It does not follow, from this mode of reasoning, that this discharge should carry with it any different mark from that which flows from the urethra; for an increased stimulus upon the internal membrane of smaller tubes will produce an increased discharge from them, alike as it does from the urethra. The more the discharge which follows the swelled testicle, the sooner will the testicle go down: and this is a reason why a swelling of the testicle, from a venereal cause, in general subsides sooner and safer than a swelling brought on from any other cause, and where no discharge follows. I am also of opinion, that when the venereal stimulus gains admittance within the orifice of the vas deferens, the progress of the inflammatory symptoms are slow there, as they are through the urethra after a gonorrhœal infection has been first received.

"From the above statement of the cause of a swelled testicle, I think we have been able rationally to account for the various circumstances attendant on the swelling of the testicle brought on by gonorrhœa—how it happens that sometimes, notwithstanding long and great inflammation in the urethra, the testicle is not affected at all—how it is from accident when it is affected—how one may be affected without the other—how that affection may come on after inflammation in the urethra has subsided—and how its own cure consists in the return of the discharge."—pp. 93—95.

In the treatment of this symptom it is necessary to suspend all the means which might suppress the discharge. If the case be slight, and the discharge continue, cooling lotions, the recumbent posture, and a suspensory bandage, may be used. If severe, general and topical bleeding (we should say the latter, employed more largely than is commonly done), fomentations and spirituous lotions, poultices and suspending the testes, keeping constantly in bed, James's powder with opium at night or an emetic, constitute the chief resources in recent cases, and camphorated mercurial ointment and poultices are very effectual in those which are protracted cases.

"*Gonorrhœa of the eye* is one of the most acute, inflammatory, painful, and destructive attacks which can be endured: in comparing it with the whole class of acute diseases that locally affect human parts, I am not able to recollect another equal to it in rapidity, torture, or destruction."

Mr. Foote goes on to contend against the opinion which ascribes it to metastasis, though he is ready to admit, that at the

period of its greatest severity the symptoms of urethral affection will be moderated, as are those of inflammation in one organ when another becomes severely inflamed. In two cases which this gentleman witnessed, "the discharge from the penis was not discontinued, but the (urethral) inflammatory symptoms were not high." Mr. F. is firmly of opinion, that every instance of virulent gonorrhœa of the eye, for we presume a milder affection, which occurs in particular individuals every time they have an urethral gonorrhœa and which does not seem to endanger the eye, he might consider as a distinct affection, connected with peculiarity of habit; but every virulent gonorrhœa of the eye, he thinks, arises from the application of the matter of *another* subject, and that this alone can account for the rarity of its occurrence, and for its being, in general, confined to one eye. This opinion is in accordance with his general belief, on the powers of matter to infect another, but not to produce fresh local affection in the subject which generates that matter, which we have endeavoured to make known, from our own conviction of its rationality and truth. One of the cases which the author has witnessed is here detailed, but our limits compel us to abstain from making an extract of it. In a note Mr. Foote gives it as his opinion, that the ophthalmia of Egypt, with which our soldiers were affected, and which they spread among their comrades on their return to England, was of the nature of gonorrhœa of the eye, and was propagated by contact, the soldiers sleeping together, washing in the same basins, and wiping with the same towels.

Gonorrhœa in women is next considered. In the female sex it is a more simple disease, from the formation of parts affected; and one of the chief difficulties attending its consideration is that of saying when the subject who has once been affected with the disease can be pronounced to be actually free from it. Mr. F. cannot remove this difficulty, and it is unfortunately too often the interest of the party to effect concealment and avoid detection, so that the imperfect means we have of forming a correct judgment are often partially or wholly withheld, and Surgeons feel a natural reluctance to pronounce an opinion which may be a serious injury to a dependant individual.

The subject of gonorrhœa we must dismiss by a brief statement of the practice of Mr. Foote. The early application of remedies is enjoined, and the best preventive of infection is said to be "the act of urining, and washing the prepuce and glans repeatedly well, and wiping them during that act." As a full bladder is not always at command, the use of a vitriolic injection, presently to be described, is recommended. This, too, used six or seven times a day, is advised, as the best injection, according to Mr. F.'s experience, to subdue gonorrhœa when its symptoms

become evident. It is intended to be employed from the very first appearance of these symptoms.

"Dissolve blue vitriol in a sufficient quantity of spring water, precipitate the solution with a sufficient quantity of lixivium tartari (which may be known by the effervescence ceasing)—suffer it to separate, and pour off the clear liquor—then wash the precipitate with warm water, set it by to subside, decant the clear liquor, and repeat the process with fresh quantities of warm water, till it become insipid and tasteless, at least of the salt—then filter the solution, and reserve the precipitate. Dissolve as much sal. volat. sal. ammon. in distilled water as it will take up, and filter it. Mix as much of the above precipitate with the filtered solution as it will dissolve, which reserve for use.

"This will be found to be a very preferable injection. I do not mean to attribute to it any other effect, than that it possesses a predominant power of extinguishing the venereal stimulus, and can be used where there is a degree of inflammation with more security and success than any thing else. I mean to offer it as a preparation which acts safe [safely] and effectual [effectually]. I mean to be understood in saying, that if it be applied properly in time, it will prevent the gonorrhœa from coming on at all; and if it be applied as soon as a gonorrhœa is discovered, it will prevent a progress of every symptom, provided the patient takes care to inject often, and observes the strictest temperance. Five drops of the fluid to an ounce of water is the proportionate medium for injection. As a preventive, it may be made stronger to wash the parts externally; a drachm of the solution to half a pint of spring water is about the proportion."—pp. 113 and 114.

Another procedure is, however, to be adopted when the symptoms have been allowed to advance, and the whole or a considerable proportion of the membrane of the urethra has become affected. Then injections of milk and water, with the application of the steam of hot water, are proper, and a moderate proportion of mercury, should be administered daily. "Four grains of blue pill, with one of opium," are prescribed, not with a view to the specific effect of mercury, but that these are found to affect the secretion of the mucous membrane favourably, and the little danger of constitutional affection which exists is by them rendered less. Mr. F. "has never known an excess of mercury abate an inflammation of the urethra;" but, on the contrary, has "always experienced the discharge protracted, and the inflammation supported from a wanton, continued use of mercury." If the symptoms increase and become more generally inflammatory, the soothing plan is to be perseveringly continued, and bleeding frequently by leeches in the perineum, the warm bath, fomentations, and James's powder at bed-time, are advised. The mercury and opium are still recommended: we question if with propriety.

The omitting to employ injections very early, or discontinuing them upon the first disappearance of the discharge, is considered to be the cause of symptoms assuming a more formidable character; but when they have become severe, the employment of the very injections which might have kept them down is forbidden. Mr. F. has evidently great confidence in the power of injections, not merely to check discharge, but in the early stage of the disease to moderate and subdue inflammation, but is anxious not to be understood to recommend them at a period when inflammation has already attained its height. Purging, *continually* employed, is condemned as more productive of protracted cases than any other plan of procedure. For the removal of gleet, bark, steel, and copaiba, are recommended.

In reciting the principal occurrences attendant upon gonorrhœa, we have anticipated the separate consideration of the treatment of each, and embodied the substance of Mr. Foote's practice with the summary of his descriptions. In women the same injection is to be employed, and Mr. F. is "confident that an early attention to consequences, and a timely precaution, will prevent the symptoms of a gonorrhœa from even coming on after an infectious connexion." After careful ablution, the injection is to be employed a few times for the first and second days after exposure to injury. When the symptoms have appeared, injection is still the chief resource. Sometimes violent inflammatory affections proceed from gonorrhœa in women, and these, of course, require active measures.

Chancre.—We have already shown the order of succession in which parts are believed by Mr. Foote to be susceptible of influence from the application of venereal virus made at one and the same time, and his opinion upon this point receives confirmation from the observation of Mr. Hunter, that gonorrhœa commonly appears sooner after the application of the poison than chancre, and that if a chancre make its appearance on the frænum, or termination of the prepuce into the glans, "the disease will, in general, appear earlier; these parts being more easily affected than either the glans, common skin of the penis, or scrotum."*

We also find that sores appearing on each side of these parts assume somewhat different characters; indeed, sufficiently so to embrace a very great proportion of the varieties of "ulceration of the genital organs," which have been named and classified by a late writer† as "not to be considered as the primary affection of syphilis," most of which *we* cannot help considering as such, though they may "not require mercury for their cure."

Before describing the characters and differences which chan-

* Hunter on the Venereal Disease, p. 218. † Mr. Evans.

cres at the present day exhibit, when affecting the various textures of the parts upon which they commonly appear, Mr. Foote endeavours to show that the disease is now what it was when it was first described by European medical writers, and has, at all intermediate periods, been the same. We entertain no doubt of its being essentially the same, and admit very readily that sometimes it has been found a most intractable disease, and that in our days it has seemed to be less formidable in its effects, at least in this country, and to the natives of some others, whilst strangers have been violently affected by it. We ask if any contagious disease which is known to have prevailed at different periods, or to have been pretty constantly occurring for ages, has not equally varied in severity from causes which we have not yet ascertained, and which may be placed beyond the reach of human penetration?

We are satisfied that the description of Marcellus Cumanus, as quoted by Astruc, and thence, perhaps, translated by Mr. Foote, together with that of Johannes de Vigo, here inserted, do sufficiently identify the disease then noted with the one still of common occurrence, which we call syphilis; and we are pleased to observe,* that in those days it was found possible to cure primary symptoms without mercury, as our army Surgeons have shown it to be in our time; and that therefore we must abandon what has unaccountably been long supposed a criterion of the nature of sores and other symptoms of a doubtful character, viz. the yielding or not to other remedies than mercury. Astruc's own description is quoted by Mr. Foote; but as we extract the author's account, we must pass by the descriptions by others which he has collected.

"When an incipient chancre is detected on the frænum, as soon as it appears, it will be a little glassy acute sore, smooth and even with the rest of the surface; it will be very sensible of a smarting pain upon being rubbed, or from slipping back the prepuce. And when it is detected behind the corona glandis, at its first appearance, it will be found to have excoriated the thin covering of the part where it is situated, and will appear also either shining, and in a small circumscribed circle, or it will inflame at some distance around it. A chancre, thus seated, will not be so painful as the former. Sometimes there will be many of them appear, at first very distinct, which will in time run into each other, just as though the virus received,

* The following is the quotation from Astruc, who is very faithful, and certainly would not misrepresent an author by needlessly giving an opinion opposed to his own:—"Duplicem distinguebat statum morbi, *recentis et confirmati*. Censebat quidem recenti convenire posse curationem *methodicam* jam supra descriptam quæ obtinebatur usu dictæ legitimæ, alterantium, phlebotomiæ, purgantium, balneorium, stuparum, et linimentorum quibuscum argentum vivum admiscebatur, *quando* ante dictâ curatione iste morbus cum suis doloribus et pustulis removeri non poterat." We could find numerous and more appropriate quotations, but do not consider it necessary.

from uncleanliness, had never been washed off, but had acted upon the cryptæ of the sebaceous glands, and had ulcerated the whole of them. When a chancre is found upon the internal part of the prepuce, it will there appear as a little pustule, such as I have described in my quotation from former authors. The cause of its appearance always as a pustule, containing a watery fluid, is, that the thin cuticle which covers the internal part of the prepuce is yet thicker than on the frænum and behind the glans, but nothing like so thick and substantial as the cuticle on the external part of the penis, but yet thick enough not to be destroyed before a chancre upon it may be detected. At the first appearance of a chancre there, it will be found to itch and to smart, and when the cuticle is destroyed, an ulcer will be seen, and the surrounding part will be somewhat thickened.

"The first appearance of chancre, both on the external part of the penis, as well as on the glans penis, will plainly point out what I am endeavouring now to make appear, namely, *that one description will not answer for the appearance of chancre on every part, it will only answer for the appearance of every chancre on parts of the same nature; and still there will be a reference to the previous condition of the natural health of the same parts.* When a chancre appears on the skin of the penis, it will be a red, hard, round pimple, with streaks of red leading from it; it will be in this state for some time, and the parts about it will appear at first thickened, but will harden also by degrees. It will sometimes not open, and yield a discharge for a considerable time after it be detected, and I have known it to remain in this indolent state for more than a month.

When it does discharge, it will be a thin ichor, and it will be found to show a strong disposition to ulcerate, and the parts around will inflame and redden. It is neither so smarting as a chancre on the internal part of the prepuce or on the frænum, and nothing like so rapid in its progress as those which I have before described. A chancre on the glans penis, considering how much that part is exposed to infection, is very rare; I mean a chancre not connected with the prepuce, but one which arises solely out of the glans. The cuticle of it will be raised, and a fluid will be discovered under it, as if it were confined within a bladder; and when this membrane is broken, and the fluid discharged, there will be a red spot remaining, which will in process of time become an ulcer, but it appears to be very slow in its action. The external orifice of the urethra will be sometimes the seat of a chancre, and which is also very slow in its action. But chancres which are found on the corona glandis are generally chancrous spreadings of venereal ulcers, which began behind the glans, and therefore their origin belongs not to the corona glandis, but to the sebaceous parts behind the glans."—pp. 147—150.

The consequences of these ulcers spreading must, of course, depend upon their seat, and the nature of the parts they involve in their course, as the prepuce, the glans, the corpora cavernosa, or the corpus spongiosum and urethra. In their progress, too,

as well as on their first appearance, chancres differ materially, according to the parts which become diseased.

"In the progression of chancres situated behind the glans, the appearance of them will be like an honey-comb: and if the same chancre spread, so as to extend to the corona glandis, which is contiguous to it, the appearances on the one and on the other will be widely different; for that part of the chancre situated on the corona glandis will not be so eaten down as the other, and will be aphthous at its base, the edges of it will bleed, and they will not thicken like the edges on the sebaceous parts. When the white aphthous appearance at the base becomes clean, the cavity, which is made by the destruction of the chancre, does not fill up, but the loss will be always apparent. In the progress of ulceration, the frænum will be often eaten away: but that which is destroyed of the prepuce will be again supplied, unless it be of very great extent, and then the prepuce will become so contracted, as to continue to be a stricture on the glans. An ulcer on this part is seldom or never aphthous, but of an honey-comb nature, and eating deep into the surface; and so will an ulcer also on the external skin of the penis."*— P. 152.

The very general, some say the constant, attendant upon a venereal ulcer is a degree of thickening of the parts surrounding it. And though the remaining of some degree of hardness in the seat of the chancre cannot always be prevented, it is in some measure an unsatisfactory circumstance. On the contrary, the entire removal of all hardness or thickening affords a fair presumption that the local infection is radically destroyed.

M. Lagneau considers the word chancre, as synonymous with venereal ulcer and syphilitic ulcer. Primary and secondary sores are by him equally included by the term, and this circumstance leads to some confusion in treating the subject. Thus, in describing the seat of the affection, he enumerates all the parts upon which primary ulcers commonly appear, and all

* If our readers will take the trouble to compare the descriptions contained in this part of Mr. Foote's work, with that given by Mr. Evans of an affection under the name of venerola vulgaris, which he says is the most common form of ulceration of the genital organs, they will not fail to find a striking resemblance. Mr. E., too, found that ven. vulg. was produced equally by gonorrhœal matter and by the matter of sores, that it was sometimes followed by constitutional symptoms, ushered in by slight fever, which in most respects resemble those of syphilis in a mild form.

Venerola indurata aut ulcus induratum, another of his species, he confesses to have "a common origin," and to be "occasionally followed by the same constitutional affections" as those of venerola vulgaris. What is this but a distinction without a difference?—a classifying, as distinct diseases, appearances which are but the varieties of one and the same disease. This is the effect of the mercurial criterion! We do not blame Mr. Evans: on the contrary, we thank him for the industry and perseverance he has shown in collecting observations, which, being faithfully made, will be valuable, when his speculations are forgotten.

those in which the ulcerations attending constitutional affection are ever seated. He describes the appearances of each more distinctly than could be expected, with such an arrangement; but in enumerating their common properties, he includes the power to produce bubo, saying, "presque tous ces ulcères peuvent donner naissance aux bubons;" as if it were quite as common an occurrence to see inflammation of a lymphatic gland from a secondary as from a primary ulcer. We ask our readers if these things be so? Then comes a difficulty which the manner of treating the subject alone raises, how to distinguish primitive from consecutive chancres, and this accompanied by an acknowledgment that the one class, the primitive, sometimes cicatrize under simple applications, whilst the other class will not commonly yield but to the regular use of antivenereal remedies. The descriptions, however, of ulcers, as they appear in the different parts, are admirably clear and correct. He notices fully the varieties of primary sores which have their seat upon the penis, and the modification to which the nature of each part of that complex structure gives rise. It is clear that he is of opinion "*that one description will not answer for the appearance of chancre on every part.*"

In considering warts of these parts, it should never be forgotten, and with regard to excoriations it should equally be remembered, that "if they be not originally venereal, they will be sure to become so, whenever a chance of contracting the disease readily offers." But, as Mr. Foote thinks, "owing to a plenteous discharge in the case of warts, other venereal symptoms, when warts are venereal, do not readily appear." "As long as a discharge continues unchecked by repellants, and as long as no mercury has been at the same time administered, I do not think other symptoms will appear; for whenever absorption does take place, either from this original cause or from chancre, the discharge from either will no longer be yielded with freedom." This remark appears to be one purely suggested by experience, and in some measure confirms us in an opinion which we entertain, that the internal use of mercury carried to a certain point, short of that at which it acts most beneficially, does for a period promote a condition of system favourable to absorption, and actually aggravates rather than appeases the venereal symptoms. We do not say that mercury, as mercury, aggravates the disease, so much as that, at the period when mercury begins to exhibit its effects upon the system, the patient is often thrown into a condition unfavourable to the salutary operation of the mercury; there is a general disturbance in which the parts affected share, absorption readily takes place—ulcers enlarging or becoming drier, and buboes then first arising, and this without the

external use of stimulating applications. Why, but for this, is there reason to say that the effects of absorption will not be produced, amongst other provisions, "as long as no mercury has at the same time been administered?" Is not this occurrence the source of many of the cases which are reported to be made worse by the use of mercury? We know of some published instances which make us think this to be the case, and we are sure that the unfavourable condition of the system, in which it is often attempted to introduce mercury, is the source of a great many more. But to return to Mr. Foote. We now have to place before our readers some of the principal opinions which he maintains respecting the local and general action of the venereal virus.

The chief proposition, and that upon which he founds most of his peculiar opinions, is—

"*That venereal fluid, produced through infection, imparted by another subject, will be harmless to the subject who secretes it. That venereal fluid, thus produced, neither generally nor particularly, will ever act to the prejudice of that subject whose secretion it is; when it appears, and when it is known, that venereal fluid of one subject, by being imparted to another subject, and by there remaining, so that such fluid may take effect, is the only means through which the venereal disease can be propagated and can be supported.*"

This proposition, as far as primary local affections are concerned, has been considered. The matter of chancres does not produce other chancres in the same subject. It is more difficult to show that the matter secreted in the original sore is not the cause of the secondary affections. Yet in the case of small-pox or cow-pox we do not ascribe the constitutional affections to the absorption of the patient's self-secreted matter, but to the action of the foreign virus; and we in no instance perceive any proportion observed between the extent of the primary and local, and the secondary and general symptoms.

It is well known that the animal matter of one subject, which has in that subject appeared perfectly healthy, and, therefore, could not but be harmless to that subject, for instance, a sound tooth has, when transferred to an absorbing surface in another person, been productive of the most severe ulcerations, and consequent train of symptoms, which have been differently termed syphilitic and pseudo-syphilitic; and whether the one appellation or the other better denote the real nature of these cases, the law of animal poisons, as here laid down in regard to syphilis, but every where, we believe, applicable to morbific poisons, is equally well supported.

Mr. Foote maintains, "that all the fluids, all the secretions" of a person constitutionally infected by syphilis, "are capable of

imparting the disease to a sound subject, provided the full opportunity be given" for the action of their infecting power.

"When the venereal poison has taken full possession of the constitution, not only the parts which indicate the disease to be venereal, by their apparent morbid vitiations, but also every part of the constitution is venereally tainted," and that by the "foreign virus alone, which, by being absorbed," brings on this "vitiation of the whole constitution."

In these opinions Mr. Foote is completely at variance with Mr. Hunter; but he shows that the experiments of Mr. Hunter, by which he thought he had proved that the matter of secondary sores is innocuous, whilst that of primary ulcers will produce chancres, not only in a person untainted, but also in one already constitutionally infected with syphilis, were fallacious, by explaining that Mr. H., in taking his matter from secondary sores, did it from the very subject on whom he made the experiments, and to whom it, according to the above law, must prove harmless; but in his trial of matter from a primary sore, took it from the chancre of another individual, and produced chancres upon the person who was already constitutionally infected. This we find to be a correct account of the manner and the result of Mr. Hunter's experiments; and, moreover, we perceive that, by inserting "the matter from a bubo of another person, where mercury had not been used," Mr. H. produced an ulcer on a woman, already constitutionally infected with lues, and which ulcer did not heal by the same applications as another slight sore which was produced by insertion of matter from her own secondary ulcers, and was cured by poultices, but did afterwards heal readily during the employment of mercury. In repeated attempts he obtained chancres, but it was from the matter of *another subject*. In this only, that we perceive, was any ulceration produced by the insertion of the individual's own matter, and in this the characters of the ulcers from the two insertions widely differed.

"So much," says Mr. Foote, "does the venereal vitiation pervade the whole of the constitution which possesses it, that even the blood of one infected, being engrafted upon a wound of another, which other was previously a sound subject, will most completely impart the disease, and will impart it with more rapidity than if it were introduced by the usual mode of infection, namely, through the absorbents."

And then he gives the celebrated instance, which was also published by Sir William Watson and by Mr. Hunter,* of the effect of transplanting a tooth. Mr. Pott witnessed the progress

* Hunter on the Venereal, p. 293, et seq. 4to. edition. London, 1788.

of the case, and "never hesitated to declare, that if ever there had been a venereal case more strongly marked by true venereal symptoms than any other, this was that case."

An instance follows of a gentleman who was infected from handling an opened body after death, he having "bitten off the cuticle which hung loose on his right fore-finger, and which generated into a chancre." The chancre did not heal, and within three months eruptions broke out over his body, which, with the chancre on his finger, gave way to mercury.

But these cases would by some persons be called pseudo-syphilitic, because cases originating in a similar way have not required mercury, or, at most, not mercury in more than alterative doses, for the removal of the symptoms. Who, however, at the present day, will venture to assert, that any venereal symptom, regular or irregular, with or without the characteristics described by Mr. Hunter, carefully treated from the first, but without mercury, will not yield to other remedies than mercury? Who is there, in short, who, if he calmly and perseveringly examines the evidence which has been accumulated, will not be staggered in his belief, if such he ever had, that there are many diseases, originating commonly in the same way, possessing so many characters in common that no one has yet succeeded to the satisfaction of more than a few partial friends in furnishing what he fancied invariably distinguishing characteristics, as to their primary appearances; and so resembling each other in consequences as to affect precisely the same parts, if not invariably in the same order, yet producing, in each and in all, very nearly resembling effects; and that these diseases are essentially and positively different? We confess ourselves, brought up as we have been in the very schools of pseudo-syphilis, prepossessed as we have been with the highest possible opinion of Mr. Hunter's and Mr. Abernethy's discernment and judgment, utterly unable to think on these subjects entirely with the former, or at all with the latter. We know very well, that the influence of such characters as these is powerful indeed, compared with our own, and that we ought, with caution and deference, to express opinions directly opposed to theirs; and, in this instance, all that we are anxious to effect is to induce those, who have not inquired fully into the subject, to examine the writings of other men, and, among the rest, of Mr. Foote, not the least ingenious, though, perhaps, the somewhat slighted opponent of Mr. Hunter. We especially wish those young men who, in their practice, feel an anxious and discouraging hesitation in determining to which of the many classes of some writers particular appearances belong, to peruse the work of Mr. Foote, and to temper any thing erroneous that it may contain, by referring to Dr. Hennen's essay above quoted.

We purposely speak of English works, because there are many readers who cannot, and some who will not peruse foreign authors on these subjects. Most continental writers who are acquainted with our pseudo-syphilitic doctrines consider that the English have embarked on a sea of conjecture, without rudder or compass to assist in the direction of their course. But to return to our task.

On the subject of bubo, Mr. F. has an excellent chapter, and in it assigns many reasons for generally concluding cases, of bubo which occur without gonorrhœa or chancre, without other evident cause, and after suspicious connexion, to be venereal. We cannot, however, follow him, and must pass over almost in total silence this and a chapter on "ulcers of the tonsils and eruptions of the skin." He thinks that after chancre or bubo, "venereal symptoms, from an infected constitution, will follow in regular succession, both in point of order and of time."

"In general, there will be a symptomatic fever, succeeded by ulcers on the tonsils, with or without eruptions on the skin, in six weeks or two months after the date of either. But when mercury has been imperfectly administered for the cure of local symptoms, the regular succession of constitutional symptoms will be interrupted; or when the virus of another subject has been absorbed into the constitution, without leaving any local impression, and in consequence of which the patient does not suspect that he is infected from that connexion, then ulcers on the tonsils, or eruptions on the skin, will appear within two or three months afterwards. This last possible form of infection being little known, and as little attended to, it has been a general practice to recur back to that period when the patient had a chancre, gonorrhœa, or bubo, and to attribute the cause of the constitutional symptoms to one of them, if it were ever so remote. But whenever I see a patient on the immediate appearance of ulcers on the tonsils, and eruptions on the skin, and who never had any local symptom, I am of opinion, that the disease was contracted by him within three months."—P. 227.

We intended to extract a long description, called a "definition of primary symptoms of lues venerea from an infected constitution," but cannot allow the space. It enumerates the forerunners of eruption—depression of spirits and indescribable languor, erratic pains in every part of the body, aching and darting pain in the cylindrical bones, distinct from the boring pains which precede and accompany nodes—restlessness and fever. All these symptoms are relieved by the appearance of the eruption. Then arise "plain spots, not protuberant, especially upon the breasts and upon both shoulders, red, purple, yellow, or livid—sometimes distinct, small, circular—sometimes broad and spreading wide"—often appearing in the hair with a scab—on the forehead and on the cheeks—at times deep

ulceration—in the hands and feet forming clefts. When the throat is affected, "the uvula, tonsils, and whole arch of the fauces suffer pain, heat, and inflammation, are eroded by ulcers," and the bones of the palate become quickly carious—or tubercles and pustules in the palate become round phagedenic ulcers, which erode the bony roof of the palate. The nasal membrane gets thickened, fungous, ulcerous, or callous. The ulcers become malignant, or the bones get involved, and the nose falls. The speech is changed, and the voice becomes hoarse. The gums are eroded, the teeth fall out, the breath is fœtid and hot, and raking.

Now this is a very formidable catalogue of severe affections, and though these be not the appearances of each individual case, could not be furnished but by a considerable number of unfortunate examples; and when we are told, in the circular of instructions sent, in 1819, by proper authority, and upon sufficient evidence, to our army medical officers, that "the reappearance of the primary ulcer, and repeated attacks of eruption, are the diseases which have been most frequently observed to succeed the non-mercurial practice," in syphilis, alike adopted in all, or very nearly all, the primary symptoms, which presented themselves, in the prosecution of a most interesting inquiry; and perceive that Mr. Foote admits, that in almost every case of secondary affection, presented in private practice, which has been otherwise neglected, from the prevailing belief of its efficacy some mercury has been used, we cannot help inquiring, how is it there should be such a difference? Let it be remembered, then, that the cases of the soldiers have been under careful management; that confinement in bed, purgatives, bloodletting, low diet, and warm baths generally, as each seemed best adapted to the constitution, and mild applications locally, have been most commonly employed.

We are apt to suspect that exposure with the infection in the system is of itself capable of aggravating the symptoms as they appear, and are convinced that when mercury is employed with exposure of this kind, or in an inflammatory condition of the system, frequently the symptoms will become severe, irregular, or intractable.

In proceeding to describe "other venereal constitutional symptoms at a more advanced stage of lues venerea," Mr. Foote first quotes a description of Vigo, who is known to have most extensively employed mercurial inunction; himself boasting "se ceratum mercuriale suâ descriptione ordinatum millies expertum fuisse cum honore et utilitate non parvâ."* He quotes also Fracastorius; and from both these writers it ap-

* Astruc. De Morbis Venereis, Lib. II. cap. vii. p. 120. 4to. Paris, 1736.

pears, that the bones were affected by the disease in their time, but were the last parts to become diseased. The erratic pains which affect them prior to the occurrence of eruptions, subside upon their appearance. The eruptions, ulcers on the tonsils, and other symptoms, still proceeding; "some time after, the pains will become more fixed, tormenting, and constant; particularly at night-time, the rest will be broken, and the disease will then begin to show itself by nodes on the periosteum, by gummata on the aponeurosis of muscles, and after these a caries of the cylindrical bones and of the head will follow."

The constitutional symptoms, it is observable, are exhibited in soft parts first, and successively appear in affections of the harder. Mr. Foote is, therefore, strongly inclined to believe, that the disease in these parts (the harder) commences with other earlier appearances; "for," says he, "I have seen them (the pains, we believe) come on from a malignant and spreading ulcer, in six weeks after the chancre healed, and they never shifted, but continued to increase for nearly twenty months, until the patient was cured by mercury in extraordinary quantities, and by many repetitions;" and Mr. F. afterwards shows, that the eruptions and other affections may be removed by a course of medicine, inadequate to the removal of affections of the bones, contending that the mercury, as does the disease, requires longer to effect a change in parts of firm structure than in those that are less firm.

Lest this opinion should induce any one to persevere uselessly in the employment of mercury, the case of Ulrich Hutton,* as given by himself, should be borne in mind. Hutton, having had the disease ten years, and having tried mercurial frictions eleven different times without permanent relief to his bone-ach, recovered his ease by taking guaiacum.

"When the pain," says Mr. Foote, "comes on upon the periosteum of a cylindrical bone, it will be found to extend the whole length of it, and to be only confined by each epiphysis of the bone. There will be exquisite tenderness experienced on touching the part, the periosteum will be thickened, and the skin which covers it will be œdematous, retaining the impression of the finger. The periosteum will at length inflame the skin, it will ulcerate and sometimes slough away, exposing thereby a caries of the bone to view. But when this is not the case, and it is not commonly so, owing to the disease not being permitted to proceed, the node will be found to be hard and circumscribed, extending itself up and down the surface of the bone, and sometimes there will be distinct nodes on the same bone."—P. 246.

The former part of this quotation is a description of periostitis in its acute form, the latter part one of node of ordinary

* See Astruc, p. 444.

and gradual formation. The distinction requires the attention of Practitioners.

We cannot follow our author where he traces the disease as affecting most of the remaining parts of the human structure, as the glands, the eyelids, the tunica conjunctiva, and the cornea. Nay, we have so much exceeded the intended extent of our analysis, that we must pass by iritis, as well as the description of the disease when affecting the ear, and Mr. Foote's admonitions never to forget the possibility of the disease recurring, after its first symptoms have disappeared.

We had purposed examining, at some length, the subject of syphilis as affecting infants, and especially to have fully analysed the paper of that excellent Surgeon, the late Mr. Hey, and the remarks upon it by Mr. Pearson in his Life of that gentleman. We must now study brevity. Mr. Hey gives several instances of mothers who have infected several children successively, without having themselves received any fresh infection in the time. He mentions a woman who was infected with a disease, which we should consider syphilis, by drawing the breasts of another woman, and who communicated the disease so caught to Mrs. B., by drawing her breasts. Mrs. B. had swelling of the axillary glands and sore throat. She was treated for syphilis, and, becoming pregnant, miscarried of a dead child. She became again pregnant, and had an apparently healthy child, which in six weeks was affected with an eruption which was judged to be syphilitic. Mercury was productive of a cure. He relates, too, the case of a lady who had some syphilitic symptoms, of which the husband appears to have suspected himself the cause, though he denied having had any symptom of the disease since his marriage. The fœtus of which she was then pregnant, when born, had an universal desquamation of the cuticle, and, subsequently, hoarseness and copper-coloured blotches. It got well by mercury. Mr. Pearson says, that a woman, "afflicted with lues venerea, may contaminate the fœtus in utero," and the child be born dead, or exhibit the symptoms of the disease in about six weeks from its birth, but adds: "It may be doubted whether an infant was ever born *alive* with the indubitable characters of lues venerea upon its body." Again; he says that the cure of the woman by mercury during utero-gestation will not protect the fœtus from the agency of the poison. Mr. P. further states having seen cases of primary symptoms in newly married women (we presume of good reputation,) whose husbands had no apparent disorder, local or constitutional, at the period of marriage nor subsequently, but *not* where neither party had previously suffered from venereal contamination. We are glad to be able to add an inference very logically deduced from some reasoning of Mr.

Pearson, that "the conclusion drawn from the operation of mercury ceases to be a certain test of the presence or absence of syphilis."*

The support which the observations and remarks of these two experienced surgeons afford to many of the opinions of Mr. Foote will not be inapparent to our readers, and we may therefore spare ourselves the pains and the space required for its elucidation. As Mr. F. believes in the power of the mother to infect the fœtus in utero, and in the capacity of persons who have been constitutionally affected, to communicate a disease of which they have no remaining evidence in their systems but this power, he could not wish to have his doctrines more remarkably confirmed.

For the removal of the symptoms of this intricate disease, mercury, Mr. Foote believes, is essentially necessary. He also thinks that no determinate quantity of mercury will with certainty effect a cure, but that in its employment due attention must be given to its effects, both upon the constitution of the patient, and upon the symptoms of the disease. He advises a *continued* effect to be produced, under confinement, with regulated diet. He admits there are constitutions which require great caution during its introduction. He prefers mercurial frictions to its internal exhibition. We must refer for his directions, which bespeak great experience and judgment. The sudden introduction of a large quantity of mercury has frequently brought disgrace upon the remedy, and disappointment to its prescriber. Its effect, supposing it to produce no worse, is to occasion sudden salivation, which continues long after the specific and beneficial effects of the mercury have ceased, and serves to deceive the patient and the surgeon. But often, too, it produces great constitutional disturbance, especially when it does not happen to cause purging; we do not mean mercurial tenesmus, but a full purging, by which, at first, its effects are lessened.

Whenever the constitution is unduly excited by the action of mercury, the local symptoms, of whatever character, as far as we have observed, whether they be primary ulcers, or affections of the fauces or nose, or palate or periosteum, are, for a time at least, aggravated, or their cure retarded.

If mercury is to be employed, we think more of preparation than is usual may very properly be practised. A much smaller quantity will answer the purpose when the system is in a favourable condition for its action. Very often when ready to condemn mercury as useless, nay, hurtful in particular cases, the use and repetition of a brisk purgative, or of blood-letting, has produced a state favourable to its action, and it has been

* Pearson's Life of Hey, Appendix II. p. 65.

resumed with quite different and beneficial effect. We lay it down, for ourselves at least, as a rule, that in all cases in which inflammatory symptoms come on (iritis excepted, and that because the organ is precious, and soon perishes,) as in recent attack of swelled prepuce inducing phymosis, in periostitis, in extending inflammation of chancres or buboes, mercury should be suspended. Antiphlogistic measures are to be adopted, sometimes with anodynes locally or generally, rest, if the case be severe, or as circumstances will, in milder instances, admit of, and perseverance in this course till the inflammation is evidently *declining*. Then mercury may be advantageously resumed, and, if cautiously used, will accelerate the decline.

The milder applications to primary sores are, so much the better chance have we of avoiding buboes and secondary affections; and we think this of more importance than a few days gained in the healing of a trifling ulcer, at the expense of the slightest hazard of the kind.

These plans we pursue in common cases when mercury is our remedy; and though far less in haste than formerly to resort to its employment, we confess that we do not often entirely disuse it in cases which we consider syphilitic. It will be perceived, too, that we consider this a comprehensive term. But we are ready enough to acknowledge, that we have now before us* evidence which, in our minds, amounts to positive proof that mercury is not absolutely necessary for the cure of symptoms of syphilis, whether they be local or constitutional. We mean here, syphilis which has proceeded from first to last uninfluenced by mercury, but under which the patient has been otherwise carefully managed, so that the disease has neither been aggravated by uncleanliness, irregularities, or exposure to atmospheric vicissitudes, nor complicated by the partial employment of mercury under unfavourable circumstances.

* Medico-Chirurgical Transactions, Vol. VIII. part 2; containing the papers of Messrs. Rose and Guthrie.

Hennen's Principles of Military Surgery, 2d edition.

Hill on the Simple Treatment of Syphilis. Edinburgh Medical and Surgical Journal, October, 1822. Cum multis aliis.

Medical and Philosophical Intelligence.

Farther Observations on the Functions of the Roots of the Spinal Nerves. By F. Magendie.—In our Number for October last,* we gave an account of a series of experiments which M. Magendie had instituted for the purpose of determining the functions of the anterior and posterior roots of the spinal nerves. By these experiments he was enabled "to assert positively, that they are possessed of different functions; that the posterior appear more particularly destined for sensibility, whilst the anterior seem more especially connected with motion." Since the appearance of M. Magendie's essay on this subject in the *Journal de Physiologie* for August last, Mr. Shaw, of Windmill Street, has endeavoured to prove that Mr. Charles Bell is entitled to the merit of the discovery. In answer to Mr. Shaw's observations, M. Magendie has inserted an article in his *Journal* for October, in which, with a degree of liberality that might act as an example to some physiologists on this side the water, he waives his claim to originality of idea, and modestly limits his pretensions to having positively established the fact, that the anterior roots are subservient to movement, whilst the posterior more particularly appertain to sensation. The following is an account of the farther experiments detailed by M. Magendie in the October Number of the *Journal de Physiologie:*—

"It is well known that the nux vomica occasions very violent convulsions both in man and animals. I was anxious to know if these convulsions would take place in a limb when its nerves of motion were divided, and if they would declare themselves as strongly as usual when its nerves of sensation were divided. The result was quite in accordance with the former; that is to say, the animal, when the posterior roots were cut, was affected with perfect tetanus, as intense as if the spinal roots had been all uninjured. On the contrary, in an animal, where I had divided the nerves of motion, the limb remained supple and motionless; at the same time that every other muscle of the body experienced the most marked tetanic contractions. I felt anxious to resolve, by experiment, whether contractions would be produced, on irritating directly the posterior spinal roots or nerves of sensation, and whether direct irritation of the nerves of motion would produce pain. With this view I commenced examining the posterior roots or nerves of sensation. The following were the results:—When these nerves were pinched, pulled, or pricked, the animal experienced pain, but its intensity was not to be compared with that which exhibits itself on touching, even lightly, the spinal marrow at the place where these roots originate. Almost every time, also, that we excite the posterior roots, contractions are produced in the muscles to which the nerves are distributed: these

* See page 162 (No. 9.) of the Journal of Foreign Medical Science and Literature.

contractions are, however, but little marked, and infinitely less weak than when we touch the marrow itself. When a fasciculus of the posterior root was divided at once, a movement was produced in the whole of the limb upon which it was distributed. I have repeated the same experiments upon the anterior fasciculi, and obtained similar results, but inversely; the contractions excited by the pinching, pricking, &c. being very strong, and even convulsive, whilst the signs of sensibility were scarcely visible. These facts, therefore, confirm those which I announced before, except that they seem to establish that sensation is not exclusively in the posterior roots, nor motion in the anterior. A difficulty, however, might still suggest itself. When, in the experiments above mentioned, the roots were divided, they were continuous with the spinal marrow; and might not the shock communicated to the latter be the true origin of the contractions, as well as of the pain which the animals experienced? In order to remove this doubt, I repeated the experiments after having separated the roots from the spinal marrow; and I must confess, that, except in two animals, which were affected with contractions when I pinched or pulled the anterior or posterior fasciculi, I observed in no case any sensible effect on irritating the anterior or posterior roots, so separated from the spinal marrow. Another species of proof still remained to be tried, viz. galvanism: the spinal nerves were excited by this agent, first in their ordinary state, and afterwards when divided at their spinal extremity in order to place them upon an insulated body. In these different cases I obtained contractions with both sorts of roots; but the contractions which followed the excitation of the anterior roots were generally much more strong and complete than those which arose when the electric current was established by the posterior. Similar phenomena took place whether the zinc or copper pole was applied to the nerve.

"I have still to give an account of the researches which I have made in endeavouring to trace sensation and motion on the other side of the roots of the nerves, viz. in the spinal marrow. I am incessantly occupied with this subject.

"Before terminating this article, it is necessary that I should make some observations respecting the novelty of the results which I have announced.

"When I wrote the paper contained in the preceding Number, I believed that I was the first who had thought of dividing the roots of the spinal nerves; but I was soon undeceived by a short note from M. Schauz (Shaw), which that young and attentive surgeon had the complaisance to send me after he received the number of my Journal. It is said in this note that Mr. Charles Bell had performed this section thirteen years before, and that he had not observed any impediment to motion when the posterior roots were divided. Mr. Shaw adds, that Mr. Charles Bell had mentioned this result in a small pamphlet printed for his friends alone, but not for publication. I immediately requested Mr. Shaw to send me the work of Mr. Bell, if possible, in order that I might render him every justice which was due to him. A few days afterwards I received it from Mr. Shaw. This pamphlet is entitled, '*Idea of a new Anatomy of the Brain,*

submitted for the observations of his friends, by Ch. Bell, F.A.S.E. (F.R.S.E.)' It is very curious, from its containing the germ of the author's recent discoveries on the nervous system. At page 22 is the passage pointed out by Mr. Shaw, which I shall transcribe:—

"'Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connexions with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, if different endowments were in the same cord, and held by the same sheath. On laying bare the roots of the spinal nerves, I found that I could cut across the fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.'

"It will be seen by this quotation from a work with which I could not be acquainted, seeing it has never been published, that Mr. Bell, conducted by his ingenious ideas on the nervous system, had well nigh discovered the functions of the spinal roots; the fact, however, that the anterior are destined for motion, whilst the posterior appertain more particularly to sensation, appears to have escaped him: I must therefore confine my pretensions to having established this fact in a positive manner."—*Journal de Physiologie, Octobre,* 1822.

On the use of Iodine.—Iodine has for some time past been frequently employed by the medical practitioners of this place, particularly among the inhabitants of a mountainous district of country, situated a few miles from Cologne, where a considerable number of cases of goitre are met with. I have, along with others, tried its medicinal effects, particularly in the cases of two sisters, the one sixteen, the other eighteen years old; the former of whom was affected with a small tumour of the bronchocele kind, about the size of a pigeon's egg, situated in the neighbourhood of the thyroid gland; the other had a chronic tumour, of much greater magnitude, situated on the left side of the neck: the swellings in both instances were hard, and quite free from pain. After the use of the iodine for a fortnight, considerable diminution in the size of the tumours was already observed. During that time the patients had only employed the Ungt. Hydriodin. made thus:—R. Axung. porc. ℥ jss. et Kali Hydriodinicum, ʒss. M. et fiat ungt. Of this ointment, about the size of a hazel-nut was rubbed in on the swelling every evening; the iodine not being at all exhibited internally. At the present moment, they still continue the use of the ointment; and I do not in the slightest degree doubt but that the tumours will be discussed without leaving the smallest trace of them, since they are already of very inconsiderable size. During the employment of this application, the patients were not affected with any particular symptom, with the exception of the younger of the two, who, about the sixth day after she began the application, complained of

slight pains in the tumour; but these entirely disappeared in two days afterwards, the use of the ointment remaining uninterrupted. Since then they have not returned.—(*Extract of a Letter from* Dr. GUNTHER, *of Cologne, to the Editor of the Medicinisch.-Chirurgische Zeitung*. 1st August, 1822.)—*London Med. and Phys. Journal.*

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Detection of Poisons.—A paragraph has appeared in the papers, recommending blue sugar-loaf paper as a test of distinction between oxalic acid and Epsom salt: it is reddened by the former, but not affected by the latter. This is perfectly true; but a simpler test consists in wetting the tip of the finger, applying it first to the supposed salt, and then to the tongue: if oxalic acid, it tastes very sour; if Epsom salt, very bitter and saline.

Many restrictions have been suggested upon the sale of arsenic; the only effectual bar to the mischief that results from it, is prohibition. It is of no use in medicine, and should therefore be rejected from the Pharmacopœia: apothecaries and druggists need not then keep it. It is sometimes employed as an instrument of research, in the chemical laboratory, which might be sufficiently supplied through other channels.—*Journal of Science.*

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Communication between the Stomach and Bladder.—The experiments made in England by Darwin, and more recently by Wollaston, Brande, and Marcet, tended to prove that different substances introduced into the stomach were found in the urine, without having passed by the medium of the lymphatics or blood-vessels. M. Fodéra has resumed these experiments; and he has given to them an ingenious modification, which enabled him to discover some phenomena which had escaped the English physiologists. He introduced into the bladder a catheter, with a cork, having tied the penis that the urine might not flow by the sides of the instrument. He laid bare the œsophagus at the anterior part of the neck, and injected into the stomach a solution containing some grains of the hydrocyanate of potass, with iron. This being done, he frequently uncorked the sound, and received the urine which flowed from it on paper. He placed a drop of solution of sulphate of iron on the paper, and added to it another of hydrochloric acid, to make the colour disappear. In one experiment, the prussiate was detected in the urine ten minutes after its injection into the stomach; and, in another, five minutes thereafter. The animals were instantly opened. The salt was found in the serum of the blood drawn from the thoracic portion of the inferior vena cava; in the right and left cavities of the heart, in the aorta, the thoracic duct, the mesenteric ganglions, the kidneys, the joints, and the mucous membrane of the bronchiæ.—*Journal de Physiologie*, Janvier, 1823.

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Transudation.—The following experiments prove that, in the dead body at least, transudation of fluids may take place at the same time from within outwards, and from without inwards, through the parietes of the intestines and of blood-vessels. M. Fodéra filled

a portion of the intestine of a rabbit with a solution of prussiate of potass, and plunged it into a solution of the hydrochlorate of lime: he introduced into it another portion of hydrochloric acid, and surrounded it with sulphuric acid; finally, he placed a bladder filled with fluid, coloured with turnsol, in a solution of galls. Some time after he found in the interior of these, respectively, hydrochlorate of lime, sulphuric acid, and gallic acid, recognised by the nitrate of silver, hydrochlorate of barytes, and sulphate of iron; and, in the liquids in which he had immersed them, prussiate of potass, hydrochloric acid, and turnsol, recognised by the sulphate of copper, nitrate of silver, and by the colour of the infusion of galls being rendered bluish by the potass.

Similar phenomena were witnessed on the living body; and the rapidity of the transudation was found to be much increased by the application of galvanism. For this purpose, the ingenious experimentalist ejected into the bladder, or into a portion of intestine, in a living rabbit, a solution of the prussiate of potass, which was made to communicate with a copper wire: he placed on the exterior surface a piece of linen, moistened with a solution of the sulphate of iron, which communicated with an iron wire: these wires were brought into contact with those of the battery. If the galvanic current be directed from without inwards, by making the iron wire communicate with the positive pole, and the copper with the negative, the tissues of the organs become imbibed with Prussian blue: but, if the current be changed, the colour is manifested on the linen.—*Ibid.*

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Hydriodate of Potass.—The importance of preparing the various salts of iodine used in medicine with accuracy, is too obvious to require comment; we therefore record the following directions respecting the hydriodate of potass as meriting attention:

One part of iodine, and three or four parts of water, are to be introduced into a phial or matrass; by degrees, and at intervals, an excess (one-half part, for example,) of pure filings of iron is to be added. The combination immediately takes place; much heat is disengaged; the iodine disappears, and the liquid becomes of a deep red colour. During this violent re-action, a hydriodate, with excess of iodine, is formed, which has only to be heated, and constantly and slightly agitated, to be converted into a simple hydriodate of iron. The action is shown to be terminated by the almost complete absence of colour in the liquid, but still more certainly by white paper being no longer stained red by it. The liquid is to be filtered, and to be augmented by some parts of water; it is to be placed in a sand-bath, in a retort or matrass, and nearly at the boiling point; then the iron is precipitated by means of pure carbonate or sub-carbonate of potass. This part of the process requires some attention, lest an excess of potass be added; which, however, may be separated by repeated crystallizations, or saturated with hydriodic acid. After having filtered, in order to separate the ferruginous precipitate, and having washed it well, it is to be

evaporated. The salt may be made to crystallize by cooling or by evaporation: in this latter case, the concentrated solution of the hydriodate of potass is to be placed, not upon a stove, because the salt would form along the sides of the vessel, and the liquid in the end be entirely remixed,—but on a very gentle fire, where the edges of the vessel, being less heated than the bottom, may condense a little of the vapour which rises, and thus prevent the ascension of the salt. By degrees, crystals are deposited; when they fill almost all the space occupied by the liquid, it must be allowed to cool; then the mother water is to be poured off, and again evaporated to obtain more of the salt; finally, the crystals are to be entirely dried at a stove or fire, where they undergo a slight decrepitation.—*Journal de Pharmacie, &c.* Janvier, 1823.

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M. Itard *on the Transmission of Sound by the Eustachian Tube.*—M. Itard controverts the received opinion, that sound is partly conveyed to the ear through the inner opening of the Eustachian tube. The experiment upon which the common opinion is founded, is, that when a watch or other sounding body is made to touch the teeth, it is heard more distinctly. It is unfortunate, M. Itard thinks, for this theory, that by putting the watch back on the tongue, without touching the teeth, and, at the same time, shutting the mouth, the sound cannot be heard at all, though the watch is much nearer the Eustachian tube. We have tried this, and find that M. Itard is correct. But why then, it will be said, do people open their mouths, when listening attentively? Perhaps to allow the sound to strike on the teeth, which are good conductors.

Itard, *Maladies de l'Oreille.*

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Dr. John Davy *on Adhesions and Coagulable Lymph.*—On injecting brandy between the lungs and pleura of a dog, firm and long adhesions were formed in twenty-four hours. This is against the received doctrine of adhesions being weak in proportion to their recentness. When blood is drawn, its albumen is liquid, but soon becomes viscid, and at length solid. When in its viscid state, it may be drawn out into transparent fibres, and bands which soon become opaque and solid, and attain their maximum of strength in a few hours. This furnishes a key to the natural history of adhesions. Dr. Baillie has committed a mistake on this point in his Morbid Anatomy, chap. i.—*Phil. Trans.*

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M. Amusat *on Fracturing Calculi in the Bladder.*—This is one of the most novel of the many methods which have been proposed to get rid of calculi, without having recourse to the dangerous operation of Lithotomy. M. Amusat has invented an instrument, consisting of pincers confined within a tube, not larger than a sound; by which, when introduced into the bladder, the stone can be easily seized, reduced to powder, and voided as gravel. A stone, the size of a nut, can be pulverized in a few seconds. It has as yet been only tried on the dead body.—*Bibliotheque Universelle.*

M. Renaud's *Case of Gastrotomy.*—A young man accidentally swallowed a silver fork, eight inches in length, in consequence of which the most threatening symptoms ensued, and the greatest alarm was entertained. Mr. Renaud, an able surgeon at Romans, (department of the Drôme,) with a view to afford the only assistance in his power to the patient, undertook the operation of gastrotomy; which he performed in the presence of several physicians of the faculty of medicine of Paris and Montpelier, and succeeded, with much dexterity, in extracting the fork. The young man is considered out of danger, and was expected to recover in a short time.—*Tablettes Universelles.*

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Mr. Travers' *Case of the Ligature of the Subclavian Artery.*—In a patient upwards of sixty years of age, in St. Thomas's hospital, Mr. Travers, assisted by Mr. Green, tied the subclavian artery, Jan. 17th, 1823. Mr. Travers made an angular incision, by letting fall a perpendicular one along the outer edge of the sterno-mastoid muscle, on the first incision made above the clavicle, and then proceeded to dissect from the flap. The great elevation of the clavicle rendered the operation very difficult, and there was a great deal of hemorrhage. The operation occupied upwards of two hours. The patient died the second or third day.—*Med. Chirurg. Mag.*

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Account of the Operation for Aneurism of the Subclavian Artery, performed by Mr. Brodie.—We had the pleasure of witnessing the operation of tying the subclavian artery, by Mr. Brodie, at St. George's hospital, on the 7th of March, 1823. There were some very interesting points in this operation, which was performed, in a very masterly style, in the space of sixteen minutes. The patient was a man about sixty, to all appearance healthy; the aneurism was not of large size, and the artery at the wrist of the affected limb, pulsated effectively, but not quite so strongly as in the other arm. The first incision was made in the usual direction, and *nearly* in a straight line, which appears to give greater facility than one more curved: the skin had previously been drawn downwards, by which the risk of wounding the external jugular was avoided. Bleeding from the numerous venous branches had always hitherto been found to obscure and retard the performance of this operation; and therefore Mr. Brodie applied a double ligature upon the external jugular vein, which vessel he afterwards divided in the interval between the two; by which means the succeeding steps of the operation were rendered comparatively easy. The ligature was placed under the artery, by means of Mr. Weiss's newly invented aneurismal needle, which here, as well as in the case lately operated upon by Mr. Travers, completely answered the expectations of the surgeon, and of the ingenuity and simplicity of which we must beg to express our most unqualified approbation. On tightening the ligature, the pulsation both in the tumour and at the wrist were immediately stopt.

We have learnt with regret that the subject of this skilful opera-

tion died on the night between the 12th and 13th, although the symptoms were so favourable at first as to lead to the expectation of a different result. We understand that, on dissection, a considerable coagulum was found in the artery, extending some way on either side of the ligature, which was separating favourably: there was neither venal nor arterial inflammation, nor any diseased appearance of the nerves. The surface of the wound was sloughy; the opposite arm and fore-arm (that is, the right, the operation having been performed on the left,) were in a state of gangrene; and, on cutting into the muscles of the limb, they were found loaded with blood, as if they had been bruised: there was also a gangrenous appearance of the left arm, near the inner condyle of the humerus, but only to a small extent.

We forbear to enter into further particulars, not doubting but that Mr. Brodie himself will detail this very interesting case more fully. We have had the satisfaction to learn that an old friend and associate, Mr. (now Dr.) Gibbes, of St. Petersburgh, has lately performed this operation with success.—*London Med. & Phys. Jour.*

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Dr. Göden *on the Seat of Puerperal Fever.*—According to the observations and dissections of Dr. Göden, the most prominent morbid appearances in puerperal fever are seated in the serous membranes. He found that the phenomena were by no means confined to the peritonæum; but were obvious also in the pleura, the pericardium, the pia mater, and the arachnoid. This, we think, is of considerable importance, if it be confirmed by subsequent observation; as it will assist us in accounting for many of the anomalous symptoms. We must confess, however, that we are somewhat sceptical as to this sweeping generalization.—*Hufeland's Journal der Heilkunde.*

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M. Dupuytren's *Case of Anomalous Pain.*—This affection occurred in a woman, whose age and temperament are not given. The pain was very similar to neuralgia, and it would have been called so, but for its seat, which was neither in any nervous trunk nor division. It appeared to affect the substance of the lips alone and their commissures, with the anterior half of the tongue on both sides. Can we from this case, and numerous similar ones, which daily occur in practice, establish an order of pains different from nervous, under the name of *pains of tissue?*—*Sceance, Acad. Roy. Med. de Paris.*

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MM. Ribes *and* Dupre *on Sulphate of Quinine in Neuralgia.*—In Neuralgia, complicated for the most part with intermittents of several varieties, MM. Ribes and Dupre appear to have been successful in the exhibition of this new medicine, the sulphate of quinine. One of the formulæ employed by Dupre, was the following:

℞. Sulphatis Quininæ gr. viij;
Syrupi Rhei.

Aquæ floris Aurantii . . . ā ā ʒij ʒiv.
Ætheris Sulphurici gutt x.
M.

The patient took three doses a day, and the sulphate of quinine was increased each consecutive day.—MAGENDIE, *Journ. de Physiologie.*

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M. SCHLEGEL *on Moxa in Phthisis.*—We are quite convinced that moxa, or in other words the actual cautery, has no specific powers of counter-irritation which may not be obtained by blistering; yet as it gives variety to the usual routine in common cases, it may perhaps be tried when patients will submit. M. Schlegel has successfully applied the moxa in phthisis, and particularly in a female, aged thirty-two, in the last stage of the disease, who was cured in two months. We must confess that we are rather sceptical as to this case.—As a substitute for moxa, since substitutes are so much the rage, why might not Dr. Chisholm's boiling water blister be tried?—HUFELAND'S *Journ. der praktischen Heilkunde.*

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Dr. GIMELLE *on Iodine in Leucorrhœa.*—Dr. Gimelle has used Iodine with much advantage in the cure of leucorrhœa of long standing, proceeding as it does from some original irritation degenerated into a state of chronic disease. He employed a syrup of iodine in the dose of an ounce night and morning, and friction every evening with a drachm of iodurated ointment. In this practice attention must be had to the peculiar effects this medicine sometimes produces on the stomach, which are best obviated by the use of acid drinks. The composition of these preparations of iodine is not mentioned.—OMODEI, *Annali.*

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M. MOSCATI *on Morbid Occlusion of the Os Uteri during Parturition.*—Occlusion of the mouth of the uterus, total or partial, may be the result of congenital malformation, or of rupture, or other injury from the employment of instruments during labour, and consequent cicatrization. Such obstruction may also arise from a change of position of the uterus in pregnancy, and the impracticability of restoring to the cervix its natural direction. Baudelocque, in such a case, unable to replace the uterus, tried to form in it an artificial opening; but the woman died from inflammation. Lauverjeat, in another instance, was more fortunate; he incised the body of the uterus on the prominence announcing the situation of the head of the fœtus, and thus procured its expulsion. The woman got well; and in two months after the uterus resumed its natural situation. But the partial occlusion of the orifice from cicatrices, consequent on laborious labour, is the more frequent occurrence. A woman, aged twenty-five, had been lacerated by imprudent application of the forceps. The uterine orifice was so much contracted by the resulting cicatrix, as only to admit a fine probe. Pregnancy again ensued; and on the occurrence of labour, forty-eight hours severe pain effected no sensible dilatation. Profiting by the

recollection of a former unfortunate case,* Moscati was deterred from the usual mode of operating, and thought of another simple means; which consisted in introducing a slightly curved *bistouri caché* into the uterine orifice, and making slight incisions round the whole circumference of the cervix, and thus enabling the orifice to yield to the necessary extent. The incisions, practised during the labour pains, caused no additional suffering. The uterine orifice, after the patient had reposed awhile, dilated circularly and uniformly; and the child and placenta were naturally expelled. Neither pain, hemorrhage, nor fever followed, and the woman speedily recovered. Still the uterine mouth did not regain its habitual softness, but presented a hard circle; an inevitable effect of the cicatrization of the numerous incisions which had been inflicted. With a view of dilating the orifice, a wax bougie, gradually increased in diameter, was for one week introduced every day, till it could no longer be borne. Eleven months after, the woman again became pregnant, and it was necessary to have recourse a second time to the bistouri to facilitate the process of parturition; but incisions, less deep than on the former occasion, were now requisite, and the operation was completely successful; as the woman, at the close of a third pregnancy, was delivered without any such assistance.—From the *Memoirs of the Italian Society of Sciences.*

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The State of the Venous Trunks in Serous Infiltrations of the Extremities.—M. Bouillaud has observed, in several cases of infiltration of the lower extremities, the venous trunks, after entering the pelvis, quite impermeable, and filled by a concrete and organized coagulum. In the case of a female, who died from chronic peritonitis and scirrhus of both ovaria, with immense infiltration of both the lower limbs, the veins, near their opening into the cava, were, in a manner, obliterated by the pressure of the diseased ovaria, and completely filled, from their junction with the cava, with a carnified and friable coagulum. In another female, aged 38, who, soon after her confinement, had great œdema of her left lower extremity, and who some time afterwards died, on dissection the veins of that limb were found to be filled with a similar coagulum to that of the former case, which extended to the vena cava. The other veins were in their usual state. The iliac portion of the colon was distended with a large mass of indurated fæces, which pressed on the adjoining veins, and gave rise to their impervious state, and to the infiltration of the limb. These observations were made in a Parisian hospital.—*Journal de Phys. Jan.* 1823.

* In this case, incision of the uterus, necessitated by occlusion of its orifice, terminated fatally. On dissection, it was found that the uterus had been ruptured to a great extent, in the direction of the angles of the incision, by the pressure of the head of the fœtus.

Obituary Notices.

DR. JENNER.

We are enabled, through the polite attention of Dr. Baron, of Gloucester, to communicate the following particulars relative to the decease of this distinguished man.

"This most eminent individual was suddenly cut off by an attack of apoplexy, on the morning of Sunday the 21st ultimo, in the 74th year of his age. The night before his seizure, he went to bed cheerful and in apparent health. He rose at his usual hour on Saturday, and came down stairs into his library. Not making his appearance at breakfast, the servant was ordered to go into his room: on doing so, he found his master lying on the floor, the head and upper part of the body being supported on the couch on which he had been sitting. Medical aid was instantly procured, and bleeding and other appropriate remedies were had recourse to without delay. In about four hours thereafter, Dr. Baron, who had been sent for from Gloucester, arrived. Every thing then denoted the rapid approach of the fatal event. The right side was paralysed; the pupil of the eye contracted to a point, and utterly insensible to light; the pulse small, and very irregular; the breathing most distressingly stertorous; and the extremities cold. Efforts were incessantly made to avert the impending blow, but they were quite unavailing, as death took place in about seventeen hours from the first seizure."

Dr. Jenner, in the early part of his career, was destined to the profession of surgery, under the tuition of John Hunter, and afterwards encouraged, by the friendship and patronage of his great preceptor, to practise that branch of the healing art in London. Preferring tranquillity to fame, Jenner declined these flattering prospects, and returned to practise as physician in his native place, Berkley, little aware how much this determination involved the interests of mankind, or his own individual reputation. He was early devoted to the study of natural history, and wrote an account of the Cuckoo, whose habits were but imperfectly known. This paper is to be found among the Transactions of the Royal Society, of which distinguished body he was soon elected a Fellow. He was the fellow-student, and in after life the friend, of the late Dr. Parry, of Bath, to whom he appears to have communicated some important observations, tending to show the dependence of angina pectoris on organic derangement of the heart. But the greatest of Jenner's discoveries, and the most important pathological fact to be found in the records of medicine, relates to Vaccination. His experiments on the cow-pox were begun in 1797; and, in the following year, with the laudable zeal of a disinterested mind, he communicated to the world a discovery, the concealment of which would have put it in his power to acquire wealth—we may almost say, without bounds. It is not our intention, however, to write a panegyric upon Dr. Jenner, knowing him, as we did, but as a public cha-

racter. The task of giving his biography, we are happy to find, is in much abler hands: that of handing down his name to posterity, he has left to no one; for, whatever the opinions with regard to the influence of vaccination may be, still, so long as the annals of our art exist, Jenner must hold a conspicuous place among the benefactors of his race.

His remains were deposited in the chancel of the parish church at Berkley, as privately as possible; but, notwithstanding the wishes of his friends, the concourse who assembled to pay the last tribute of respect to this great philanthropist, was immense. From the general feeling, as well as what past in the House of Commons on the subject, it is to be presumed that a national monument will be erected to his memory. We observe with pleasure that a meeting was held at Gloucester, on Saturday, the 22d ultimo, at which it was resolved to erect a monument in the neighbourhood of that city; the expense of which is to be defrayed by private subscription.

Dr. Baron of Gloucester, who long enjoyed his confidential friendship, is to become his biographer and the publisher of numerous manuscripts, which are placed in his hands for that purpose. To this work we therefore look forward with confidence, satisfied of the authenticity of the documents, and the talent which will be employed in their arrangement. Meantime we subjoin a reference to some of Dr. Jenner's most important writings:

History of the Cuckoo,—(Philosophical Transactions.)

Inquiry relative to the Origin of the Variola Vaccina.

Observations on the Interference of Herpetic Eruptions with the regular Progress of the Vaccine Vesicle.

Letter to Dr. C. Parry on Artificial Eruptions.

Dr. Jenner likewise published some papers relative to Vaccination in different periodical works. Accounts of these, as they appeared, are to be found in the various Numbers of this Journal, which was established at the time Dr. Jenner's discovery began to attract notice. We refer our readers to volumes i. iii. iv. v. vii. viii. xii. xiii. and xxii.

The last paper published by Dr. Jenner, and which contains his latest opinions on some points connected with vaccination, is to be found in the Number of this Journal for September, 1822.—*Lond. Med. and Phys. Journ., March*, 1823.

DR. MARCET.

Died, in London, on the 18th October, at the age of fifty-two, Dr. Alexander Marcet. The death of this excellent physician has occasioned a more intense degree of regret, than has been occasioned by the demise of any medical character in London for a great length of time. In Dr. M.'s death, the younger members of the profession have lost a counsellor and protector, and the elder ones a friend. He was long physician to Guy's Hospital, and effected changes in its management, of which his successors now reap the benefit. He was principally instrumental, not only in

forming, but also in supporting the greatest and most useful medical associations in this or in any other country. The numerous works he has left behind him, attest his industry and talents.—*Quarterly Journ. For. Med. and Surgery, April*, 1823.

DR. PETT.

Too much caution cannot be enjoined as to post mortem examinations, and in this point of view, we think, our readers would do well to peruse M. Gaspard's experiments on the absorption of putrid substances. (*See Magendie's Journ. Janvier* 1823, *and Quart. Journ. For. Med.* xiv. 223.) Dr. Pett, we are informed by Mr. Travers, assisted a medical friend in examining the body of a lady, who died of peritoneal inflammation after child-birth. Twelve hours after, he complained of pain in the middle finger of his right hand, where a slight superficial wound was discovered. This was first touched with caustic, and afterwards with strong sulphuric acid, but he did not feel either of the applications. A second application of the lunar caustic produced intense pain. This was followed by severe rigor, and the pain spread with increasing agony along the arm. He passed a sleepless night, and in the morning his finger was white and without sensation, and his countenance alarmingly altered. The arm went on to swell, the superficial absorbents appeared inflamed, the pectoral and axillary region became much affected, the finger put on the appearance of gangrene, and there was high nervous excitement generally. The unfavourable symptoms rapidly increased, and notwithstanding every means of remedy, he sunk in 105 hours after the injury. On examination, the chest and abdomen were found healthy, the heart rather large and flabby, and the liver considerably deranged by a chronic affection.—*Ibid.*

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Died, early last month, William Blair, Esq., late Surgeon to the Lock Hospital.

Died, lately, Dr. George Gilbert Currey, senior Physician to St. Thomas's Hospital.—*Ibid.*

QUARTERLY LIST OF NEW FOREIGN MEDICAL PUBLICATIONS.

Conspectus Medicinæ Theoreticæ; or a View of the Theory of Medicine. By the late James Gregory, M.D., formerly Professor of the Theory of Medicine, afterwards of the Practice of Medicine in the University of Edinburgh. In two Parts. Part I. containing Physiology and Pathology; Part II. containing Therapeutics. Translated from the original Latin. Edin. 1823. 8vo. pp. 535.

A Practical Treatise on the Bath Waters, tending to illustrate their beneficial Effects in Chronic Diseases, particularly in Gout, Rheumatism, Paralysis, Lead Colic, Indigestion, Biliary Affections, and Uterine and Cutaneous Diseases; confirmed by Cases: con-

taining likewise a brief Account of the City of Bath, and of Hot Springs. By Joseph Hume Spry, Surgeon, &c. Lond. 1822. 8vo. pp. 442.

Leçons sur les Epidémies et l'Hygiène Publique, faites à la Faculté de Médecine de Strasbourg. Par Fr. Emm. Foderé, Professeur à cette Faculté. Tome premier. A Paris, 1822. 8vo. pp. 523.

Sur l'Origine des Qualités Morales et des Facultés Intellectuelles de l'Homme, et sur les Conditions de leur Manifestation. Par F. J. Gall. Tomes 1 et 2. Paris, 1822.

Thoughts on the present Character and Constitution of the Medical Profession. By T. C. Speer, M.D., M.R.I.A., late Physician to the Dublin Dispensary, &c. Cambridge, 1822. 8vo. pp. 132.

Nouveaux Principes de Chirurgie, rédigés suivant le Plan de l'Ouvrage de G. de la Fage; contenant, 1. Une Introduction à l'Etude de la Zoonomie, l'Anatomie générale, l'Anatomie descriptive et la Physiologie; 2. L'Hygiène; 3. La Pathologie générale; 4. La Pathologie externe ou chirurgicale; 5. Enfin, la Thérapeutique, la Matière Médicale, et les petites Opérations de la Chirurgie. Par F. M. V. Legouas, D.M.P., etc. Quatrième édition, revue et augmentée, &c.

Histoire de l'Epidémié de Suette-Miliare qui a régné, en 1821, dans les Départemens de l'Oise et de Seine-et-Oise. Par P. Rayer, Docteur en Médecine de la Faculté de Paris, Médecin titulaire du Quatrième Dispensaire, &c. 8vo. Paris, 1822. pp. 480, avec deux planches lithographiées.

Exposition Méthodique du Règne Végétal, dans laquelle les Plantes sont classées d'après les Différences qu'elles présentent dans leur Organisation, et leurs Fonctions; précédée d'un Mémoire sur les Fruits, et d'un Tableau Systématique de tous les Etres Organisés. Par J. F. Caffin, Médecin. Un vol. in 8vo. Paris.

An Exposition of the Principles of Pathology, and of the Treatment of Diseases. By Daniel Pring, M.D., Member of the Royal College of Surgeons, London. 8vo. pp. 522. London, 1823.

A Treatise on Indigestion and its Consequences, called Nervous and Bilious Complaints; with Observations on the Organic Diseases in which they sometimes terminate. By A. P. W. Philip, M.D., F.R.S. Ed., &c. Third edition, with some additional Observations. 8vo. pp. 409. London, 1823.

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